

The Journal of the INSTITUTE OF METALS



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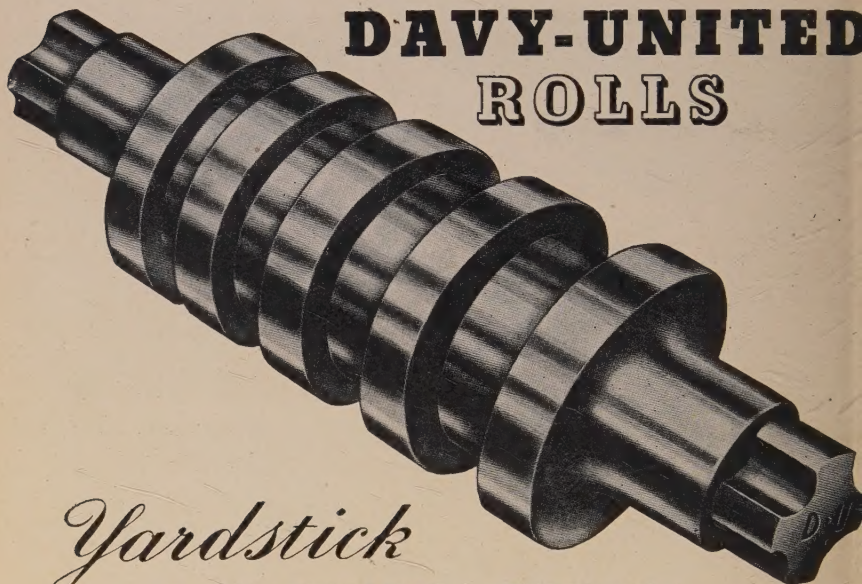


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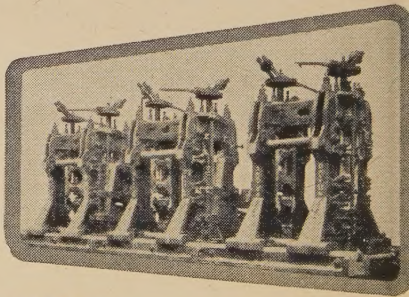
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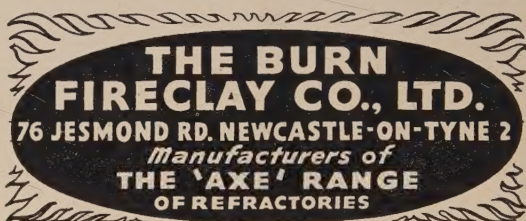
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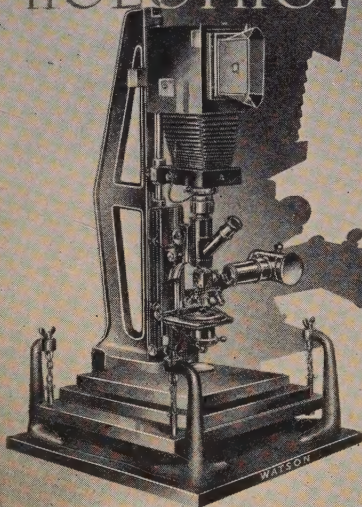
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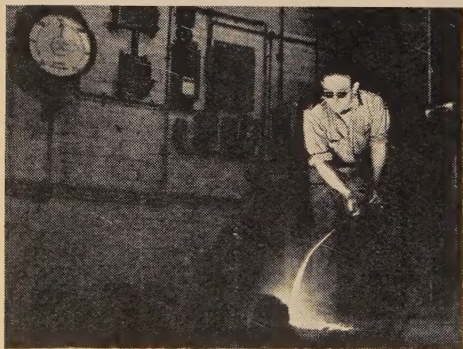
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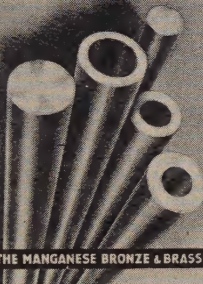
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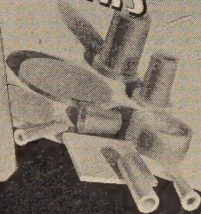
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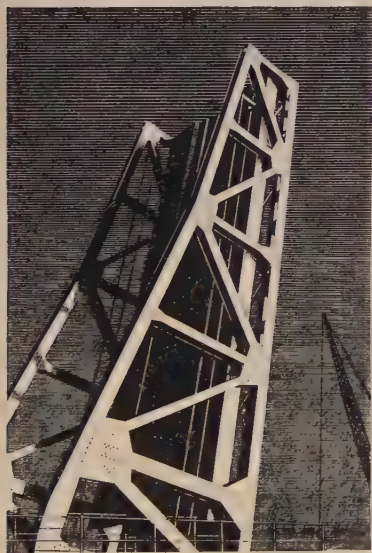
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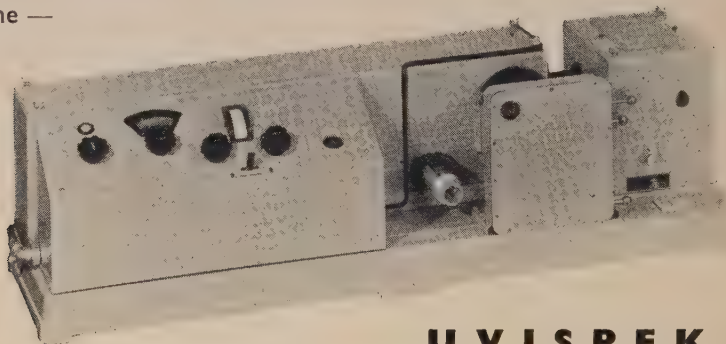
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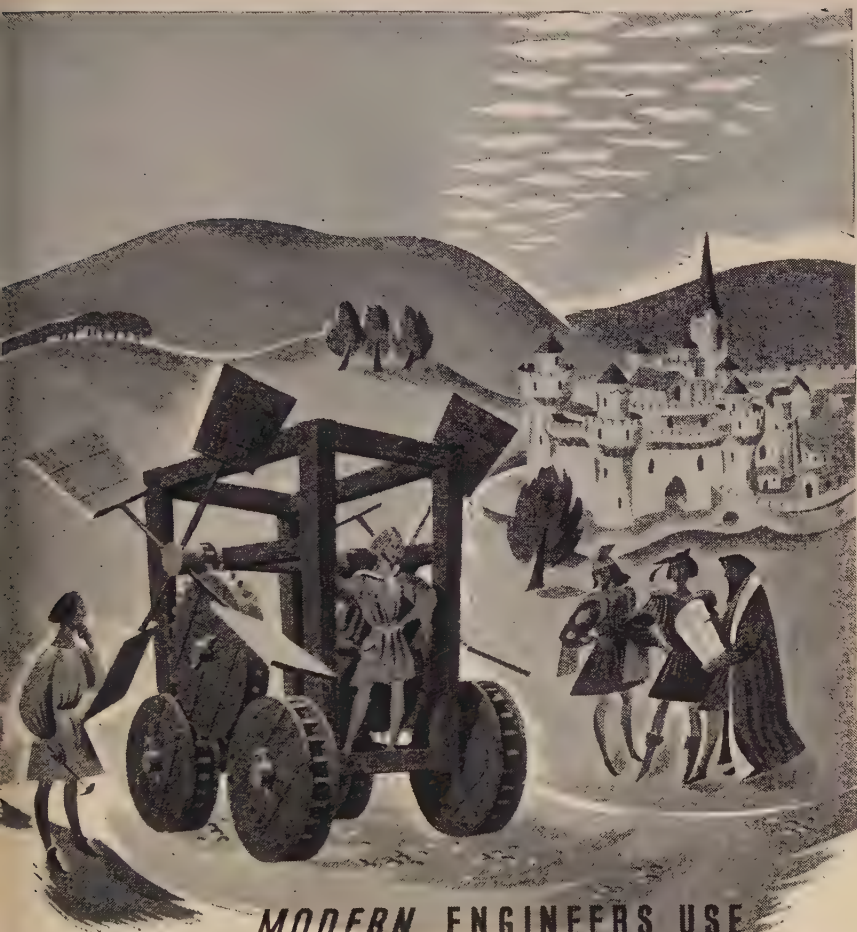
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CORRESPONDENCE ON PAPER BY MR. H. T. GREENAWAY : " THE SURFACE TENSION AND DENSITY OF LEAD-ANTIMONY AND CADMIUM-ANTIMONY ALLOYS."

(*J. Inst. Metals*, this volume, p. 133.)

DR. S. W. SMITH,* C.B.E., A.R.S.M. (Member) : The author's reason for following up the work of previous investigators, namely that the data obtained should have considerable practical and theoretical significance, is, I think, fully justified. Similar beliefs have been expressed in the past.† Libman,‡ in 1928, gave full expression both to the practical and to the theoretical importance of surface-tension data as "a growing necessity". These views have been reiterated by Vance White and others in more recent years.

Why is it, one is inclined to ask, that expectations entertained and expressed so long ago, do not appear to have yielded the practical and theoretical applications which were anticipated? Is it, perhaps, that those who have given consideration to these matters have tended to focus their attention on capillary phenomena in terms of surface tension rather than on the simpler consideration of capillarity itself as a measurable unit? Libman made a comment which is pertinent to this question. He said "in most capillary phenomena and in any practical case, the governing factor is the capillary constant of the metal". This is usually denoted by a^2 , the product of two linear measurements—the depth in millimetres to which a molten metal would be depressed in a non-wettable tube, and the radius of the tube itself in millimetres. Since values for surface tensions, by whatever methods obtained, can be readily expressed in terms of capillary constants, it would have been useful, for practical purposes, if the author had followed the course of previous workers and given in separate columns the corresponding values of a^2 . From his results for the surface tensions of the pure metals, it will be found that the corresponding values for a^2 are in general conformity with those shown by previous workers.

By following up earlier work on capillarity and surface tension it may appear to physicists that the author has undertaken a task which seems likely to yield figures which can only be critically regarded as approximations to complete accuracy. But the value of such approximations in what is undoubtedly an extremely difficult field of metallurgical investigation, should not be under-estimated. A mere glance at the efforts made by others, from those of Quincke 90 years ago to those of the present author, serves to emphasize the manipulative difficulties which are presented.

It is precisely because of these difficulties of manipulation which the author so frankly records, that some doubts are raised with regard to the inferences he draws on matters of constitutional and theoretical interest, to which, in the present stage of his work, he may be attaching undue importance. One of these is the possible significance, or otherwise, in regard to molten metals, of deviations from the so-called "Macleod's constant". While such deviations in the case of certain organic liquids, over relatively small ranges of temperature, have been used by Sugden as a criterion of what is regarded as "association" in those liquids, it is hardly true to say that Bircumshaw assumed that

* London. † *J. Inst. Metals*, 1914, 12, 168; 1917, 18, 66.

‡ *Univ. Illinois Bull.*, 1928, (173).

Sugden's findings would apply to liquid metals, although he did, reservedly, indicate that the acceptance of such a criterion might furnish evidence of such behaviour.

It seems doubtful, too, whether the change of gradient and departure from linearity observed by the author in the curve of surface tension when plotted against composition in a system in which an intermetallic compound is known to exist in the solid state, can be regarded as evidence of the persistence of the compound in the liquid phase, having regard to the experimental difficulties encountered, to the different ranges of surface tension in the two systems examined, and to the fact that in one case the curves are plotted for values at 50° C. above the liquidus and in the other case at 140° C. above the liquidus. For similar reasons, the suggestion that the surface tension of cadmium has a large positive temperature coefficient from its melting point upwards for 40° C., changing to one of slightly negative sign beyond that temperature, is one which should be regarded with reserve, conflicting, as it does, with so much work which supports the general belief that surface tension diminishes with rising temperature. Hogness recorded a negative coefficient for cadmium from the melting point upwards, and Bircumshaw's figures over a wide range of temperature (330°–600° C.) differ so little that it would seem unjustifiable to draw a conclusion which is contrary to the general experience with other metals.

Finally, some comments may be offered regarding the manipulative difficulties which were encountered and which the author has so frankly recorded.

Of the many different methods of approaching the problem of measuring surface tension, the author has selected that of the so-called "Maximum Bubble Pressure" to which Bircumshaw devoted many years of careful work at the National Physical Laboratory, following, in principle, the earlier work of Hogness, in which, however, small drops of metal were exuded under pressure from the tip of a capillary tube. There were difficulties and limitations in both cases, particularly in applying the methods to metals other than those of comparatively low melting point.

An essential part of the apparatus which the author describes is, of course, the concentric tubes from which the bubbles are released. The character of these tubes—whether of silica or stainless steel—is clearly an important factor, and one gathers that in view of the unsatisfactory behaviour of stainless steel, the observations were made mainly with silica tubes. The degree of polish to which the ends of these tubes were subjected was also a determining factor in obtaining consistent results, and the criterion of the right degree of polish seems, in part at least, to have been the degree of conformity to previously published results. There was some difference in regard to the effect of polish between his experience and that of Bircumshaw, who found that a finer polish to silica tubes gave higher rather than lower results. The degree of polish would also appear to have some bearing on whether the inner or the outer diameter of the tube—or some intermediate figure—should be taken in determining the area from which the bubbles are detached.

But the most serious doubts of all are raised by the author's reference to the drossing which occurred in the case of cadmium and of its alloys with antimony. In the density determinations he also found that stainless tubes were severely attacked. Such drossing and corrosive action would seem to imply oxidation of the metal and a consequent "wetting" of the tubes and a change in the interfacial tensions. Vance White showed the disastrous quantitative effects which oxidation may have. The preference shown by the author for an atmosphere of hydrogen, rather than one of nitrogen, seems to indicate that hydrogen exerted some effect, at least, in preventing excessive oxidation. But the necessity which he found for resorting to the provision of a flux to

minimize the trouble, seems, on the face of it, to have been an undesirable expedient and a source of danger. The true solution of the difficulty would seem to be in more stringent precautions to prevent any possible contamination of the metal.

The difficulties experienced also in regard to the lead-antimony system recall similar difficulties encountered by the writer in determining capillary depressions in carbon tubes in the lower ranges of temperatures for *all* metals which are readily susceptible to crossing, unless the utmost care is taken to prevent the least degree of oxidation. This could only be attained under strongly reducing conditions. The author mentions the steps taken to dry and to deoxidize the gases before admission to the apparatus, but his diagram (Fig. 1) does not indicate the point at which the water manometer intervened to measure the pressures. Is it possible that water vapour from the manometer had access to the molten metal?

It is for these various reasons that hesitation arises in accepting the author's values for surface tensions as more than useful approximations, from which it might be misleading to draw those theoretical inferences to which he attaches importance in Section V of his paper.

The adaptation, however, which he has made of his apparatus to the determinations of the liquid densities of molten metals is indeed a new and useful contribution to the experimental side of work to which many previous workers have made contributions. The values obtained are in close agreement with those given by Pascal and Jouniaux, Hogness, and others who employed more laborious methods. The author gives the external diameter of the tubes as 2.5–3.0 mm., but he might usefully have added the internal diameters as well. It would then be possible to say to what depth below the surface the upper tube should be immersed in order to make quite sure that the surface tensions *do*, in fact, cancel out. In the case of cadmium, for instance, taking his own determination of the surface tension, it would be essential that the depth to which the end of the upper tube should be immersed below the surface of the metal should exceed 15 mm. if capillary effects are to be avoided—assuming that the internal diameter of the tube is of the order of 2 mm. (i.e. 1 mm. radius).

If the author could extend his work—both on surface tension and on liquid density—to metals and alloys of still higher melting point, regarding which data are still very meagre, a substantial step would be made towards a better understanding and a closer control of operations which involve capillary phenomena in molten material undergoing solidification.

The AUTHOR (*in reply*): I agree that the data obtained by the various workers on the surface tension of liquid metals have not yielded the number of practical and theoretical results expected, although some industrial practices are now more readily understood by reference to surface-tension values, e.g. soldering and casting.

The values of the capillary constants (a^2) could quite well have been included, but were omitted from the rather lengthy tables of results for the sake of brevity, as it was felt that anyone interested could readily calculate these values.

With reference to the inferences which I drew from the increases in Macleod's constant found for all liquids investigated: these were prefixed by the statement "If the constancy of this relation (Macleod's constant) is accepted as a criterion of an unassociated liquid, then . . .", and were never meant to be anything more than possibilities. Bircumshaw went no further than this in his work.

The difference in ranges of surface tension of the two systems examined is appreciable, but I am not sure that it has any bearing on the conclusion drawn

as to the persistence of the intermetallic compound in the liquid cadmium-antimony alloys. A comparison of the surface tension-composition relationships obtained by Sauerwald and Drath for the bismuth-tin system and those obtained by myself for the cadmium-antimony system, shows that the ranges of surface-tension values are comparable; yet the relationships for bismuth-tin (with no intermetallic compound) are approximately linear, whereas those for cadmium-antimony (with an intermetallic compound) show a large departure from linearity. Also, in the case of lead-antimony alloys, if one were to plot composition against surface tension at liquidus temperatures or 50° C. above (as already done for cadmium-antimony alloys), one would obtain (for those alloys for which values were determined) a curve of similar slope to that obtained at 140° C. above the liquidus. The objections raised on these counts therefore seem invalid.

With reference to the surface tension of cadmium, the determinations showing a large positive temperature coefficient for 40° C. above the melting point appeared quite satisfactory and were therefore given in the paper. They certainly conflict with the general belief that surface tension diminishes with rising temperature, but they do agree with Bircumshaw, who states: "Hogness, in his paper, remarks 'the surface tension of cadmium appears to go through a maximum'. This remarkable result has been definitely confirmed in the present work, and apparently liquid cadmium is the only pure liquid known in which surface tension rises with temperature." These remarks conflict with those of Dr. Smith.

Matsuyama's results for the surface tension of cadmium also indicate that there may be a maximum value at about 400° C. Sauerwald and Drath reported that copper and some of its alloys show a positive temperature coefficient. These results seem to indicate that cadmium and copper (at least) are exceptions to the rule.

The published observations, with the exception of those for mercury and water, were made using silica tubes. The criterion of the right degree of polish on the ends of the tubes was regularity of results and agreement between values obtained for tubes of silica, stainless steel, and glass, rather than conformity to previously published results. As I stated in the paper, there is some difference between the results of Bircumshaw and myself regarding the effect of polish of silica tubes. This difference has not been explained.

The outer walls of the tubes were taken as those on which the bubbles formed since: (a) The tubes should not be wetted by the metal, in which case the outer diameter is the reasonable one to use in the calculations. Sauerwald and Drath satisfied themselves that this assumption was correct. In the apparatus used, the bubbles were blown well under the surface of the metal, and hence should not be affected by any slight surface dressing. The attack noted by some alloys on the tubes occurred at the metal-air surface, and does seem to imply oxidation of the metal and a consequent "wetting" of the tubes, but only at this surface. (b) Calculations based on the outer diameters of tubes of various sizes of the one material or of different materials gave the same result, while calculations based on the inner diameters of these same tubes varied widely. (c) Damage to the inner wall of a tube did not alter the result obtained, whereas damage to the outer wall did.

The better results obtained in some cases using hydrogen rather than nitrogen were due, I think, to a slight content of residual oxygen in the nitrogen, which would cause some oxidation of the metal. The provision of a flux for the cadmium alloys was perhaps undesirable, but should have been satisfactory unless it contaminated the metal at the source of the bubbles. No such contamination was detected by chemical tests.

The difficulties experienced in obtaining consistent maximum bubble pressures for low-antimony lead-antimony alloys near their melting points do

not appear to be due to drossing, as suggested by Dr. Smith, since any effect of drossing would be expected to increase with temperature.

The apparatus was set up in such a way that water vapour from the manometer should not have entered the tube assembly from which the bubbles were blown. Care was also taken to ensure that the tubes (in both density and surface-tension work) were sufficiently immersed in the metal under test.

Measurements of density and surface tension in higher temperature ranges should be quite feasible using the methods employed in the present work, if suitable high-melting-point oxide refractory tubes (silica, alumina, beryllia, &c.) and carefully controlled atmospheres (inert gases or hydrogen) are used. Because of other programmes at present in hand, it is not intended to carry this work any further for the present.

FURTHER CORRESPONDENCE ON PAPER BY MR. N. DUDZINSKI, MISS J. R. MURRAY, MR. B. W. MOTT, AND DR. B. CHALMERS: "THE YOUNG'S MODULUS OF SOME ALUMINIUM ALLOYS."

(*J. Inst. Metals*, this volume, pp. 291, 685.)

DR. H. J. AXON,* B.Met. (Student Member): In discussing this paper, it is of interest to consider first the systems aluminium-silicon and aluminium-beryllium. In these systems it is logical to extrapolate the authors' experimental results to 100% of alloying element. The work of Middleton, referred to in the text of the paper, indicates an approximately linear relation between the Young's modulus of an alloy system and the proportions of the metallographic constituents present. By analogy with the concept of a clamped-up aggregation of perforated plates, mentioned by Professor Aitchison in the discussion at the Annual General Meeting, it would appear that the linear relationship should hold good between the Young's modulus and the *volume-%* of the metallographic constituents.

Raynor, however, in the oral discussion, has postulated a linear relation between Young's modulus and *atoms-%*, and the authors have presented their experimental results in terms of *weight-%*. In an attempt to clear up an uncertainty I have re-plotted the experimental data in terms of volume-, atoms-, and weight-% for the two systems aluminium-silicon and aluminium-beryllium and have extrapolated the Young's modulus to 100% of beryllium and silicon. The values so obtained are tabulated below, together with the experimentally determined values for the pure elements beryllium and silicon.

TABLE A.

Extrapolated Value When Plotted Against---	Young's Modulus for Silicon.	Young's Modulus for Beryllium.
Volume-%	18	26
Atoms-%	19.5	18
Weight-%	18	35
Young's modulus of the pure elements .	16	40

From Table A it may be seen that the linear relationship holds best when the data are plotted in terms of weight-%. This conclusion is somewhat surprising, but may explain the discrepancy between the heat of formation of FeAl_3 and the Young's modulus in aluminium-iron alloys, mentioned by Mr. Dudzinski in the oral discussion.

In the absence of any experimental data on the Young's modulus of pure intermetallic compounds, it is impossible to conduct a further check on the linearity relationship. It is, however, probable that for compounds formed

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between aluminium and the transition elements, the Young's modulus values will be very similar, since the Young's modulus gives an indication of the tightness with which atoms are bound together in the solid state.

The slight difference of Young's modulus values which has been noted in the aluminium-silicon system in the modified and unmodified state may well be associated with the influence of the modifying agent on the Young's modulus of silicon rather than with the size of the crystals of silicon.

MR. N. DUDZINSKI (*in reply*): Dr. Axon refers to a linear relationship that should hold good between the Young's modulus and the volume-% of the metallographic constituent, as indicated by the work of Middleton.*

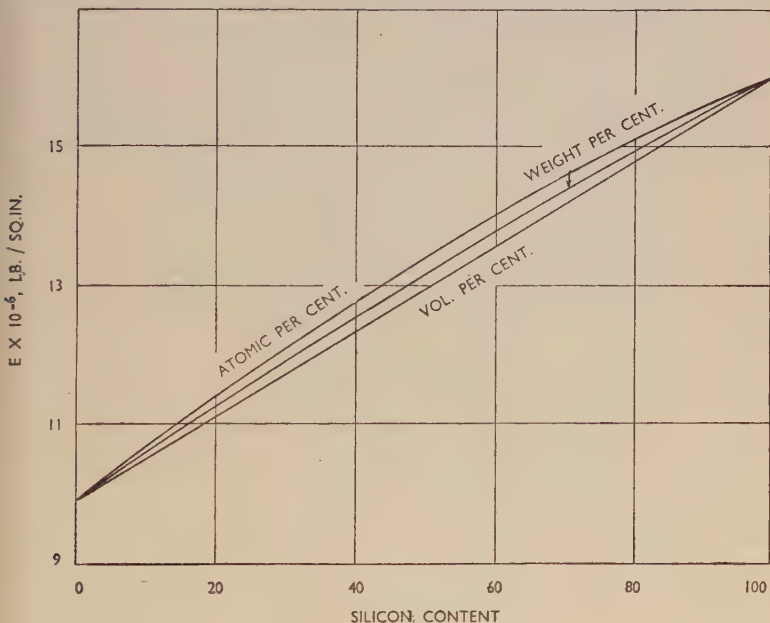


FIG. G.

As we investigated aluminium alloys with rather small concentrations of the solute, we did not pay much attention to the method of graphical representation of the results, knowing that the deviation from the straight line would be small whether we plotted the values of Young's modulus against the concentration in weight-, atomic-, or volume-%. We used the weight percentage as it appeared the most convenient.

Middleton calculated the value of E for an aluminium alloy with 12% silicon and obtained a figure of 10.8×10^6 lb./in.²; this was in agreement with the experimental result. He was using the following expression:

$$E = \frac{nw'}{[w'' + n(w' - w'')]} \cdot E'' + \frac{(1 - n)w''}{[w'' + n(w' - w'')]} \cdot E'$$

where E , E' , E'' are the values of Young's modulus of the alloy, matrix, and

* B. F. Middleton, *R.A.E. Tech. Note No. M9546*, 1945 (Sep.).

constituent respectively, w' and w'' are the densities of the matrix and constituent respectively, and \bar{n} is the weight fraction of constituent, i.e. $100\bar{n}$ = the weight-% of constituent in the alloy.

Using the expression above, I have calculated the Young's modulus of two series of binary aluminium alloys, one with silicon and the other with beryllium. The results of these calculations are shown in Figs. G and H, in which the values of E are plotted against the concentration of silicon or beryllium expressed in weight-, volume-, or atomic-%. In the case of the aluminium-silicon system the curves are very close to each other as the differences in atomic weights and densities of these metals are small; the

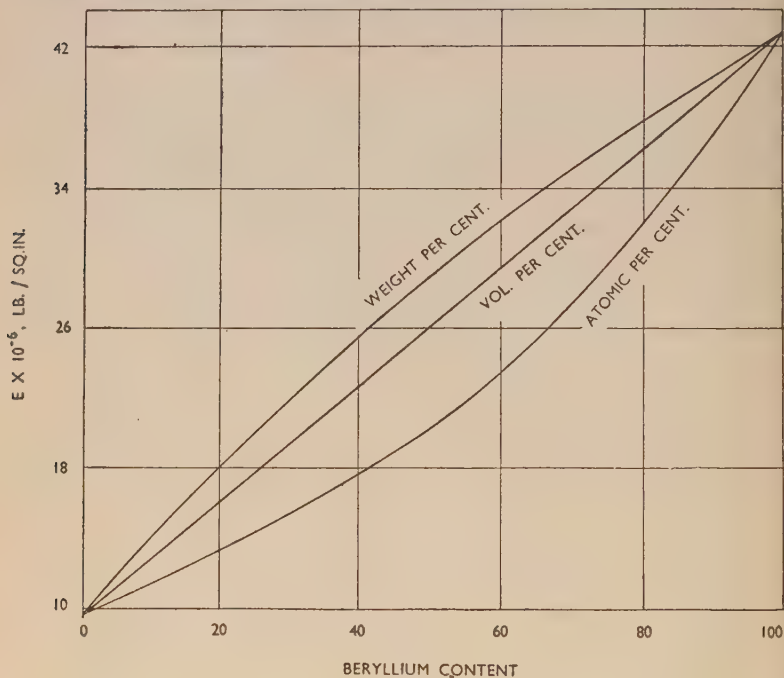


FIG. H.

curve representing a relationship between E and the volume-% is a straight line.

For the alloys of aluminium with beryllium, the values of Young's modulus plotted against the volume-% of beryllium assume a linear relationship, whereas if the beryllium content is expressed in weight- or atomic-% the curves show deviations from the straight line, particularly marked in the case of the latter.

With the object of applying Middleton's expression to the alloys in which intermetallic compounds are formed, I prepared an aluminium alloy with 54% copper, which corresponds to 100% CuAl_2 . The specific gravity of this alloy has been determined and the Young's modulus value extrapolated from the increment in E per 1 wt.-% of copper content.* Calculated values for

* N. Dudzinski, *R.A.E. Rep. No. Met. 36*, 1948 (Sep.).

Young's modulus have been plotted in Fig. J against the CuAl_2 or copper content in weight- or volume-%. A linear relationship is shown when the values of E are plotted against the concentration in volume-%; in the case of weight-% there are deviations from the straight line. This does not agree with our experiments; however, the difference is quite small. The extensive work of Guillet* showed a linear relationship between the Young's modulus and the concentration of alloying element in weight-% in the copper-aluminium, copper-zinc, and copper-tin systems.

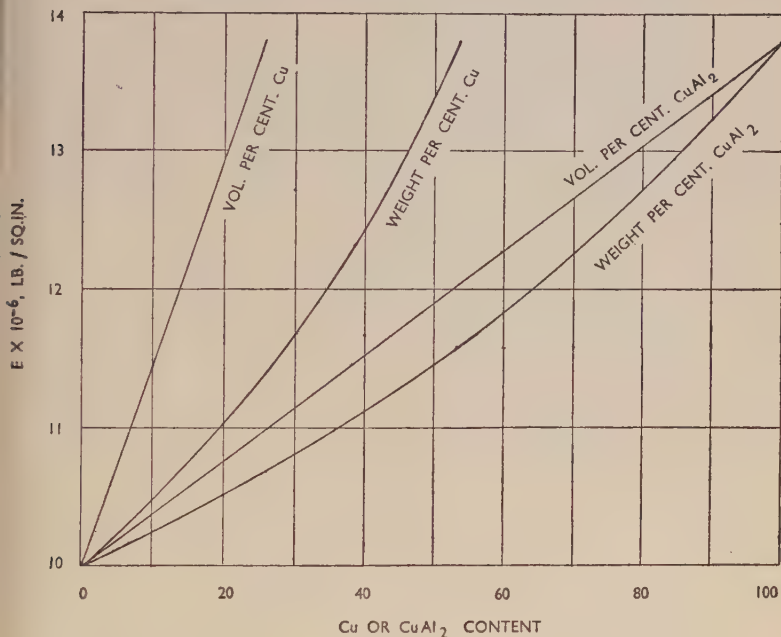


FIG. J.

Dr. Raynor, in the oral discussion, presented his figures on the heat of formation in the terms of atomic-%, as this property is related to the gramme-atom. For direct comparison it would be necessary to convert his curves into weight-%, but the deviation from linearity of such curves for the small concentration of solute would be small.

Regarding the slight difference in values of Young's modulus which has been observed in the aluminium-silicon alloys in the modified and unmodified states, it is not likely that this is associated with the influence of the modifying agent on the Young's modulus of silicon.

Slightly higher values of Young's modulus observed in aluminium-silicon alloys can be attributed to an increase in density of these alloys after modification treatment. It is of interest to add that a linear relationship has been observed between the Young's modulus and the values of specific volume in a number of binary aluminium alloys.

* L. Guillet, Jr., *Thesis*; Univ. Paris, 1939, 74 pp.

DISCUSSION ON PAPER BY MR. G. A. COTTELL, DR. K. M. ENTWISTLE, AND PROF. F. C. THOMPSON: "THE MEASUREMENT OF THE DAMPING CAPACITY OF METALS IN TORSIONAL VIBRATION." *

(*J. Inst. Metals*, this volume, p. 373.)

MR. G. A. COTTELL, in presenting the paper, said: The conclusion of the work described in the paper leaves two important questions unanswered. Firstly, the exact nature of the damping-frequency relationship for Duralumin is uncertain; this matter is being investigated further. Secondly, measurements with the small machine have so far been confined to Duralumin and alpha-brass, and the relative magnitude of the damping capacity of other metals, when tested in this machine and by the physical method, remains to be determined.

While it is considered that the apparatus loss in the small machine is of a low order in comparison with the damping capacity of low-damping materials, in its present form it suffers from two rather serious disadvantages. In the first place, as a result of the manner of gripping, the specimen is unavoidably destroyed in removal. The design of a form of grip which will permit a specimen of easily machinable shape to be removed and replaced without introducing a grip loss presents many difficult problems. Secondly, the method of strain measurement is insufficiently sensitive to enable accurate results to be obtained for some metals in the very low strain range where the damping capacity is independent of amplitude, a range which appears best suited to investigations of structural condition. In the case of annealed copper, for instance, the maximum strain under these conditions would be less than 10^{-5} , corresponding to a scale deflection of only one millimetre when using the present method of strain measurement. Further development work is needed in these directions.

Since this paper was published, it has been pointed out to us that when calculating the strain values in the tests with the *small* machine we have disregarded the fact that a reflected ray of light is deflected through twice the angle of rotation of the reflecting surface. The result is that the strain figures given in the paper, both in the diagrams and in the text, are just twice what they should be. As this regrettable error does not affect significantly either the test results or the conclusions, we may perhaps be allowed to claim that, like the servant's baby, it is only a little one and may therefore be forgiven. The full list of corrections is as follows:

p. 376, Fig. 1. Divide strain-ordinate figures by two for curves VIII and IX only.

p. 398, Fig. 15. Key. Remarks. Substitute for $\theta = 0.054$ to $\theta = 0.045$, the following: $\theta = 0.027$ to $\theta = 0.0225$.

p. 400, Fig. 16. Divide strain-ordinate figures by two. Key to Fig. 16 (p. 401) under heading "Pressure":

<i>For</i>	<i>Substitute</i>
$Pa = 0.00273 + 0.006 \theta$	$Pa = 0.00273 + 0.012 \theta$
$Pa = 0.0034_2 + 0.006 \theta$	$Pa = 0.0034_2 + 0.012 \theta$
$Pa = 0.0040 + 0.0072 \theta$	$Pa = 0.0040 + 0.0144 \theta$
$Pa = 0.00456 + 0.013 \theta$	$Pa = 0.00456 + 0.026 \theta$
$Pa = 0.0067_8 + 0.021 \theta$	$Pa = 0.0067_8 + 0.042 \theta$

* Discussion at the Annual Autumn Meeting, Cambridge, 15 September 1948.

- p. 401. Substitute for C_8 : $Pa = 0.0034 + 0.0058 \theta$ at 747 cycles per minute, and C_1 : $Pa = 0.0034 + 0.0087 \theta$ at 738 cycles per minute, the following:
 C_8 : $Pa = 0.0034 + 0.0116 \theta$ at 747 cycles per minute, C_1 : $Pa = 0.0034 + 0.0174 \theta$ at 738 cycles per minute.
- p. 401. 6. *Air Loss Over Wheel of Large Machine*, 6th line. Substitute for $\theta = 0.02$ radians, $\theta = 0.01$ radians.
- p. 402, Fig. 17. Key. Curve II. For amplitude (θ) = 0.02 radians, substitute (θ) = 0.01 radians.
- p. 406, Fig. 18. Divide strain values by two.

DR. R. F. HANSTOCK* (Member): It is a remarkable achievement that the authors have been able to improve the accuracy of what might be described as an historical mechanical device for measuring damping capacity, to the extent revealed in this paper. One is justified, perhaps, in referring to this device as historical, because it is essentially a simple torsional pendulum, and as such was used at least as early as 1865 by Kelvin to investigate damping effects in metals. A specialized form of this pendulum, designed by Pertz, was described by Föppl in a lecture to the Iron and Steel Institute in this country in 1936. This lecture aroused considerable interest, because Föppl made some very definite statements concerning the practical importance to designers and engineers of the inherent damping capacity of structural materials.

Generally speaking, this instrument as designed by Pertz showed that, while materials differed considerably in their damping capacity, values of less than about 0.5% were seldom found at stresses of a few tons/in.² As at that time there was no direct evidence to suggest that those values might be abnormal, Föppl's contention that the damping capacity of the material could be an important factor in the control of resonance vibration was largely substantiated.

Following the development in more recent years of physical methods of measuring damping capacity at stresses of several tons/in.², it became apparent that the Föppl-Pertz machine, certainly for aluminium alloys, gave values which were far too high. The thoroughness with which the authors have detected and eliminated sources of error in the original Föppl-Pertz instrument is evident in Fig. 1 of their paper, and it is interesting to note that in the course of their investigation they have eliminated many of the features of the original design. In fact, the small instrument which the authors prefer is a simple torsional pendulum shorn of the electromagnets, stylus, and recording drum of the original Föppl-Pertz instrument, and is an excellent illustration of the fact that accuracy is often inherent in simplicity.

What is perhaps more astonishing is that the only precaution which was considered necessary in the original Föppl-Pertz machine to prevent loss of vibrational energy, namely suspension of the whole machine by a comparatively slender wire, has been shown to be more of a nuisance than a necessity.

The entire paper shows the determination of the authors not to accept any preconceived ideas concerning the effect of certain constructional details on the accuracy of measurement of damping capacity. However, they have not succeeded solely by removing unnecessary parts of the Föppl-Pertz instrument; by careful experiment they have discovered how to fix one piece of metal into another so that no significant loss of vibrational energy occurs at the joint.

As a result of this work, there is now no serious discrepancy between values of damping capacity measured by the two chief methods, at least on one type of material. Both methods are capable of detecting a diminution of vibrational energy per cycle of less than 1 part in 100,000. The two methods are not competitive; they are really complementary; and I have no doubt that

* Chief Physicist, High Duty Alloys, Ltd., Slough, Bucks.

the instrument perfected by the authors will soon provide valuable information concerning damping capacity.

I feel that the most useful contribution that I can make to the discussion is not to comment any further on specific details of technique, but to ask a general question. Ten years ago there were good reasons to suppose that damping capacity was of sufficient practical importance to justify its measurement merely in order to have an indication of the suitability of certain types of material for the construction of components which might be subjected to vibrational stresses. Now, however, the authors confirm that the damping capacities of some materials are very much lower than the Föppl-Pertz machine indicated them to be. It is true that the error for other classes of material may be smaller, and this point obviously requires further investigation, but even so it seems reasonable to consider whether damping capacity is still significant solely in respect of its magnitude.

Having perfected the instrument, does the original need for it, as envisaged by Föppl, still exist, or alternatively are there any other reasons for supposing that the measurement of damping capacity is desirable from the practical point of view? By asking these questions I do not wish to imply that the answers are in the negative, but, even if they were, the authors are entitled to our admiration for a paper of outstanding value.

DR. H. SUTTON * (Member): I am very pleased to have the opportunity of saying how much I personally admire and appreciate this excellent piece of work by the authors. They have shown quite clearly and definitely the importance of particular factors which had been the subject of considerable doubt in the minds of those of us who are interested in damping studies.

The results of this work are of particular interest to mechanical engineers. It had been hoped from the earliest studies of damping that most engineering structures benefited a great deal from mechanical damping, and that view is strongly emphasized by the work which the present authors have done and have placed on record so carefully for us.

Many of us are interested in vibrational problems in relation to high-speed air-current effects, and it is very gratifying to know that air damping can have such a powerful effect relative to material damping under the conditions of the authors' experiments. I am sure that the data which the authors have put on record will prove of interest and value to those concerned with aerodynamic damping.

There is another very important question, that of frequency. I gather that when writing the paper the authors felt that they had not yet accumulated sufficient data to enable them to form any really firm conclusions about the general effect of frequency. I hope that they will have the time and the opportunity to go ahead with studies of frequency, and I look forward very much to hearing of that further work.

There are others more competent to speak on a more important aspect of damping studies, namely damping as a means of gaining an insight into the changes which are going on within materials. In that field the work of Dr. Cyril Smith and his colleagues in Chicago has been of very great importance and influence in the scientific world. I hope, therefore, that those who have made a special study of that field, including Dr. Cyril Smith, will have something to say on that side of the matter.

MR. L. ROTHERHAM,† M.Sc., F.Inst.P. (Member): First of all, I should

* Deputy Director of Research and Development, Aircraft Materials, Ministry of Supply, London.

† Head, Metallurgical Department, Royal Aircraft Establishment, Farnborough, Hants.

like to say that I am sure that everyone here will agree with me in applauding the diligence with which Professor Thompson and his colleagues have applied themselves to the study of the sources of error in the measurement of damping capacity.

I read the paper very carefully, and it was not until I reached the conclusions that I began to have any doubt at all. I had no doubt at all even then about the experimental work which is described, but in the first paragraph of their discussion, p. 409, they say that "the measured damping values obtained with a machine based on the Föppl-Pertz design are no indication of the true damping capacity of the material under test". Those are strong words. I am not prepared to argue the case for the Föppl-Pertz type of machine, but by implication I feel that the authors suggest that all measurements in the so-called mechanical type of machine are necessarily rather inaccurate.

I do not think that the evidence which is presented justifies that conclusion, and, since possibly the largest amount of data recorded on that type of machine is that due to the late Dr. Hatfield and myself and other colleagues, I shall have to make some comparisons with the technique which we used.

Some of the improvements which were introduced into Professor Thompson's machine were used throughout our experiments. We never used the machine suspended on a thin wire, but always rigidly clamped to a large mass. Right from the start the steady-bearing was eliminated. We never used any results unless we could show that by taking the specimen out of the machine and replacing it upside down or rotating it through 90° , it gave consistent results. In one respect I think that the design was superior to the original Föppl-Pertz design, in that we suspended the torsional pendulum from the top instead of the bottom, which is a condition of more stable equilibrium. Even so, in those tests we had a good deal of trouble from bending of the specimens, and if I had not seen Mr. Cottell in action I should ask how it was prevented in his work.

The results which we published included a good many results for two 0.2% (approx.) carbon steels, which may be compared with the results given in Fig. 4 of the present paper. The amendment of the results there which Mr. Cottell has indicated in introducing the paper may invalidate the comparison which I have made, but I do not think that it does. A comparison of our data with those published by the authors shows that the results which we obtained correspond very closely with those given in curves VII and VIII in their Fig. 4; that is, they are the same as those obtained by the authors when the modifications to the original machine were complete, but before eliminating air-frictional effects. Further comparison of our results is difficult, since the Duralumin-type material which we used was in the form of sand-cast bars. The kind of errors which are left after the modification of the large machine are those which are indicated by Fig. 7 of the paper, which gives the results on an aluminium alloy. It seems probable that our results may have been in error by an increment on the damping capacity of 0.1-0.2%, which is similar to that shown by curve II, Fig. 7. We always said that there might be a residual error of the order of 0.1%. An error of this magnitude would account for our failure to observe any substantial reduction on testing *in vacuo*.

The general conclusion which I reach, therefore, on reading the paper is that the technique which we used in those experiments was not bad. The interesting thing about damping capacity, of course, is not so much the absolute magnitude of the effect, but the way in which it varies with temperature and frequency. I believe that, allowing for an error of the kind suggested, our observations on the effect of temperature were significant.

Some reference has been made to the additional experiments which the authors propose to make or have made on the effect of frequency on the damp-

ing capacity of Duralumin or of the aluminium alloy in question, R.R. 56. I am very glad of that, because as it stands at the moment it is still rather misleading to suggest that the two methods of measuring damping capacity are measuring the same property. The authors are well aware, from their own observations as well as from published work, of the fact that damping capacity is extremely sensitive to changes in frequency, and I do not think that it can be too strongly emphasized that a test at 149 cycles per minute is not the same thing as a test at 48,240 cycles per minute. In the present case they happen to give the same values, but in other cases they would differ by a factor of 10 or 100.

I congratulate the authors on a most careful piece of work. I have still some more work to do on damping capacity, and I shall take heed of the lessons given in this paper.

DR. CYRIL S. SMITH,* B.Sc. (Member) : Since Dr. Sutton has referred to the work at the University of Chicago, perhaps it would not be inappropriate to give a brief summary of the work being carried out there by Dr. Zener and his associates, Drs. Kê, Dijkstra, and Wert.

Dr. Zener has been interested in the use of internal friction as a tool to study many different aspects of the behaviour of metals. In particular, it has been shown that internal friction in non-ferromagnetic materials can arise from time-dependent stress relaxation due to at least six different mechanisms, as follows † : (1) intercrystalline thermal currents (depending on elastic anisotropy of adjacent grains); (2) transverse thermal currents (depending on heat flow from the compression to the tension side of a bent bar); (3) interstitial solute atom segregation (the slight tetragonality resulting from segregation of interstitial atoms under stress relaxes to random cubic structures on relief of stress); (4) migration of twin boundaries, particularly in the case of twinning in tetragonal structures having an axial ratio of close to 1.00; (5) grain-boundary slip (due to viscous shear across grain boundaries); and (6) solute atom segregation (a result of the slight tendency of large atoms even in substitutional solid solutions to align with the direction of stress).

These are roughly in decreasing order of rate of relaxation at a given temperature. Internal friction due to each of these causes is strongly dependent on time and hence on frequency. It is important for the engineer to realize that internal friction is not a single simple property of the material, but that it varies greatly with frequency of oscillation of the stress, temperature, and often with the prior stress history and the presence of minor constituents in solution or otherwise.

DR. C. J. SMITHELLS,‡ M.C. (Member of Council) : I should like to ask Dr. Smith whether there is not another factor in ferromagnetic materials, due to magnetic hysteresis, which presumably is quite important.

DR. SMITH : Yes.

DR. SMITHELLS : My reason for speaking is that about two years ago the Research Council of the British Non-Ferrous Metals Research Association discussed whether they ought not to take an interest in damping capacity. There was considerable difference of opinion, as we were divided into those who felt that damping capacity was an important property and those who doubted whether there was such a thing. I, being the greatest doubter, was made

* Director, Institute for the Study of Metals, University of Chicago.

† See C. Zener, "Elasticity and Anelasticity of Metals". Chicago : 1948. (University of Chicago Press.)

‡ Director of Research, British Aluminium Co., Ltd., Gerrards Cross, Bucks.

Chairman of the Committee afterwards formed to encourage Professor Thompson to do the work which he has been doing, and which he was doing for a long time before we started encouraging him.

At the time that this Committee was formed to follow the work of the team at Manchester, the results that were being obtained varied by about 1000 to 1 according to how damping capacity was measured, so that we had some justification for our doubts. Looking at it from the engineering point of view, if damping capacity was of the order of 1% it might be of some interest to designers, but if it was of the order of 0.001% it seemed doubtful whether it would come in as a factor in design. When one considers how difficult it is to get rid of all the extraneous damping in any machine used to measure this property, it seems likely that in an engineering structure, damping from mechanical joints and so on is so much greater that intrinsic damping capacity hardly comes into the picture.

We did realize, however, that even if it was of no importance in engineering design, it was likely to be an important research tool, and we decided that it was a subject which was of importance and one which the Association would be justified in supporting. This was only about two years ago, and the progress made by Professor Thompson's team in that time has been remarkable.

We have been able to go to Manchester and see from time to time the work going on there, and the progress has been rapid, and the mechanical difficulties that Mr. Cottell and other members of the team have had to tackle have been considerable.

I have no criticisms to offer on the paper. The work of the authors has removed a great deal of the uncertainty regarding measurements made by the mechanical method, and results are now in close agreement with those obtained by physical methods. There is no doubt that damping capacity varies with structural changes in a metal, but to be of any use as a research tool it must be reliable. The authors have by their work removed many of our doubts about the reliability of the measurements themselves, and have therefore provided a tool which is going to be of real use.

In fact, they have already gone much further than the work recorded in this paper, and have produced results, which I hope will be published in due course, which show how useful a tool it can be.

DR. G. L. J. BAILEY,* B.Sc. (Member): This paper may very well stand as a model of the rational approach to the solution of a difficult and initially obscure problem of measurement. The first comment that comes to mind is that it is no longer permissible to rely on the damping capacity of the materials used in engineering design for limitation of the amplitudes reached in resonant vibration. Almost certainly, the internal friction at low stress amplitudes is less, and may be a great deal less, than has been thought, and the chance of building up vibrations of larger amplitude correspondingly greater. The work of Hanstock and Murray has shown that increased damping at higher stresses may well mean that the material has already begun to fail.

The authors' experience of grip losses suggests that the damping capacity of joints is comparatively much larger, and might repay study as a function of the materials and the methods of making joints, from the engineer's point of view at any rate.

Reference to the work of Hanstock and Murray in the Introduction to the paper raises a point on which some clarification seems to be desirable. For a material in a given condition there is apparently a range of stress amplitude from zero upwards in which the damping capacity is independent of the stress.

* Head of Physics Section, British Non-Ferrous Metals Research Association, London.

Within this range, the measured value is independent of the number of cycles of vibration undergone, and will not change unless the material changes structurally, for instance by ageing. On the other hand, if the stress is high enough to disturb that structure, the damping value depends on the vibration history of the specimen. One must, therefore, be careful to distinguish an "intrinsic" damping capacity, characteristic of the material in a given condition, from the high-amplitude value which depends on the vibration history. The intrinsic value is the proper one for studying the effects of grain-size, of progress of transformations, for example, and the high-amplitude value is of use mainly in the study of fatigue characteristics.

The authors suggest the use of damping-capacity measurements as a quick non-destructive quality-control test, capable of sensitive indication of structural changes, and quote Zener's work showing the relationship between damping capacity and grain-size as example. However, Zener and his colleagues use damping-capacity measurements as one means of obtaining experimental evidence on their views on the inter-relation between the mechanical properties of metals and their structural arrangement, and their work shows the fundamental significance of the property. It may well be that there is a field for the empirical study of intrinsic damping capacity as a function of the specimen material and its condition, leaving the explanations until later, and that from such a study useful quality-control tests will emerge. In such work the measurement of damping *versus* frequency at low amplitudes would be more useful than the measurement of damping *versus* amplitude at a particular frequency, which has often been done in the past.

I think that the authors would agree that the machine in its present form has some way to go before quick non-destructive testing becomes possible with its aid.

In conclusion, I should like to refer to the authors' remarks on p. 413 on the possible frequency dependence of the internal friction of Duralumin. Would they expect to find a frequency-dependent contribution to the damping as a result of intergranular thermal currents in this case, since aluminium is elastically almost isotropic?

DR. K. M. ENTWISTLE (*in reply*): I gather from the contributions to the discussion that there are three main points which are exercising the minds of the audience at present. First of all, the questions asked by Dr. Hanstock: "Why measure damping capacity anyway? Is it any use from an engineering point of view?" Secondly, the uncertain nature of the variation with frequency of the damping capacity of Duralumin referred to by Dr. Sutton and Dr. Bailey. Thirdly, there is the query raised by Mr. Rotherham in connection with the comparison between the old types of machine and that which we have now produced. In the limited time available, I propose to deal only with these three points.

To deal first with the question of frequency, in some work which has been done since this paper was published, we have interested ourselves in the effect of grain-size on the damping capacity of α brass in torsional vibration. Broadly speaking, we have extended the measurements made by Dr. Zener and his colleagues in longitudinal vibration mentioned by Dr. Smith. Dr. Zener predicted that there would be a contribution to damping capacity arising from intercrystalline thermal currents in all types of vibration, and we have found experimentally that this is the case. The important implication of this result is that, in any material which is elastically anisotropic, the damping capacity will vary with frequency, since the contribution from intercrystalline thermal currents is frequency-sensitive. I feel certain that when we investigate the behaviour of Duralumin over a wide range of frequency, we shall find a limited but measurable, range of variation arising from the same effect. As Dr

Bailey pointed out, the elastic anisotropy of aluminium, and therefore, presumably, of Duralumin, is very small; it is finite, however, although the resulting effect will be smaller than is the case with α brass. This may explain the difference between the results given by the small machine and, at higher frequencies, by the physical method. Other effects may be operating, especially at the lower frequencies.

Dr. Sutton drew some consolation from the fact that there seemed to be appreciable aerodynamic damping resulting from vibration in air, but I would point out that the magnitude of this energy loss is not very large. The aerodynamic damping in the large machine is about 0.004%. In the case of turbine blades, vibrating presumably transversely, our experience indicates that the damping will be higher, but probably will not exceed 0.01%, which corresponds to an energy loss per cycle of only one part in 10,000. I do not think that damping of this magnitude can be relied on to limit the amplitude of resonant vibrations, and, personally, I feel that a better solution to the problem is to get away from the resonant frequencies rather than to rely on the damping of the material itself.

In connection with Mr. Rotherham's contribution, I would point out to him that the opening part of the discussion on p. 409 reads: ". . . in the case of materials of relatively low damping capacity, the measured damping values obtained with a machine based on the Föppl-Pertz design are no indication of the true damping capacity of the material under test". That is, we consider that reliable results cannot be obtained on low-damping-capacity materials, but we draw no conclusions as to the values obtained for materials of higher damping capacity, which, presumably, will be less influenced by apparatus loss. The particular Föppl-Pertz type of machine on which we based our comparison with the small machine was that existing in the Department of Metallurgy at Manchester University at the time this work was begun, and which is regarded as a typical example of the Föppl-Pertz design.

We are in agreement with Dr. Smith's summary of the contributions to internal friction arising from diffusion processes, and consider that, of the factors he enumerated, damping from intercrystalline thermal currents is the one which is most likely to explain the difference between the higher- and lower-frequency test results.

In connection with Dr. Hanstock's remarks, I should like to say a word about the measurement of damping capacity from a metallurgical point of view. We are interested in the relationship between the structure of a metal and the magnitude of the internal friction at extremely low stresses, where the damping capacity is independent of both the number of stress cycles and the strain amplitude. We consider that, under these conditions, it is possible to obtain a direct indication of the structure of the material; that is, the process of measurement does not, in itself, affect the structural condition which is being investigated. Dr. Hanstock's work shows that high vibrational strains can cause a variation in the structure of the material, or, more rigorously, the vibrational strains produce changes of damping capacity which are similar to those resulting from changes of structure. The two fields are separate; the first uses low-stress measurements to indicate the metallurgical condition, and the other is concerned with the effect of high vibrational strain. Both are important, and a great deal of work remains to be done in each direction.

CORRESPONDENCE.

MR. J. SAVAGE* and MR. M. W. THRING†: In the past, the study of the damping of vibrations in metals appears to have followed two diverging lines. On the one hand the instrument initiated by Pertz and Föppl has led to a commercial instrument which has been used for routine control tests of metal specimens. On the other hand, scientific investigations which were concerned with the actual causes of damping of metals, i.e. whether this was caused mainly by flow at the crystal boundaries, by heat flow, or by absorption of energy by viscous processes within the crystals themselves, have used different instruments according to the range of frequencies to be studied.

It would seem that one of the reasons why this paper is of particular value is that it enables these two lines of research to be brought together again, since it is now possible to design an instrument which is convenient enough for routine testing, while at the same time the major errors are eliminated so that its results may be used for the fundamental study of the internal behaviour of the metal. One would like to ask the authors whether, as a result of their experience, they would recommend that future control work should be done with the improved large instrument, and future fundamental work, where torsional vibrations are concerned, with the small instrument in a vacuum, or whether they would recommend the small instrument, used without a vacuum but with appropriate corrections, in both cases. In the Physics Department of B.I.S.R.A. we are particularly interested in this work, as we hope to be able to apply the apparatus developed by the authors of this paper to ferrous metals. In particular, the relation of the damping capacity to fatigue along the lines studied by Hanstock is obviously an important line of research for ferrous metals. Changes of damping capacity during transformations in steels would also be a very interesting study.

A question which seems to arise, now that a standard method of measuring damping capacity has been developed, is the relation between this property and the specific absorption of supersonic waves in the material, since if this relation turns out to be a fairly close one, as one might expect, then it might be possible to calibrate the absorption of supersonic waves by means of damping-capacity tests on specimens of suitable shape, and subsequently to use the absorption of supersonics as a non-destructive test for the onset of fatigue.

The authors refer to frequencies in the range 150–1200 cycles per minute. The lower frequency range would appear to be rather high for the investigation of the damping due to the diffusion of carbon in iron, studied by Snoek and Polder, but presumably it would be possible to modify the apparatus by a wheel of greater inertia so as to reduce the frequency about three times and thus study this specially interesting effect which occurs in steel.

In conclusion, we would like to stress again the great benefit to all those working on other metals of having the fundamental considerations in the damping-capacity apparatus worked out for us so thoroughly as it has been by the authors.

The AUTHORS (*in further, written, reply to the discussion and in reply to correspondence*): In reply to both Dr. Bailey and Dr. Hanstock we would stress that we do not regard it as implicit in the results of our work that materials of high damping capacity at low stress do not exist. We consider that such materials as manganese bronze and cast iron will still show a high damping capacity when tested in the small machine within the stress range

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allowed in engineering design. The engineer, therefore, may have available materials which can be relied on to contribute towards the energy absorption of vibrating systems of which they form a part. The highly strain-sensitive damping capacity in ferrous materials, even below the stress at which fatigue failure is possible, may again result in a significant contribution to vibration limitation. In the case of the low-damping-capacity materials which we have investigated, however, Dr. Bailey's comment is substantially true, but further measurements with the small machine are required to settle the question in the case of other metals which we have not tested.

The question raised by Dr. Sutton concerning the magnitude of aerodynamic damping can be answered in full only by a research programme designed specifically to give the required information. Our experience indicates that air damping will be of secondary importance in most engineering vibration problems, and at high temperatures its magnitude will become less important on account of the possible reduction of air-friction damping coupled with an increase in the damping capacity of the material itself. Questions of this nature, together with the important factor discussed by Dr. Bailey—energy absorption at joints—are of fundamental importance in vibration problems and are a profitable field for future engineering research.

We cannot agree with the suggestion made by Mr. Rotherham that measurement of the variation of damping capacity is of greater interest than its absolute magnitude. In our opinion it is impossible to determine the former other than by measurement of the latter. It cannot be assumed that the apparatus loss is independent of such factors as frequency, temperature, specimen size, and material. Therefore the determination of the true variation of damping capacity with, for example, frequency or temperature, can only be carried out with apparatus in which the external losses have been either eliminated or assessed experimentally for the particular test conditions. The elimination of apparatus loss is essential for the detection of slight variations of damping capacity. For example, we have found that during the ageing of Duralumin the measured damping capacity changed from a value of 0.002% to a maximum value of 0.0035%; an apparatus loss of 0.1% would obscure this 0.0015% variation, while a loss of 0.01% would result in a considerable decrease in the accuracy with which it could be measured.

Dr. Bailey suggests the definition of an "intrinsic" damping capacity. We have used the same term during the development of the apparatus in a different sense, to distinguish that part of the measured damping capacity contributed by the specimen itself. In this sense contributions at both high and low strains are intrinsic. We agree that, for structural investigations, measurements at low stress are in general the more suitable, while the higher-amplitude contributions are particularly interesting to the engineer. It is difficult to draw a dividing line between the two, however, since the work of Hanstock has shown that measurements at higher stresses also have significance with regard to metallurgical condition; and, in fact, the contributions to the damping capacity manifest at higher stresses are theoretically present, although on a much reduced scale, at low stress. We agree with Dr. Bailey that it is legitimate to single out damping capacity measured at low stress as specially important, but we do not consider the adjective "intrinsic" to be appropriate. It is interesting to record that in recent years this damping capacity has been designated "damping capacity at vanishing amplitude"—a more lengthy but appropriate description.

The authors agree that quality-control tests should be preceded by fundamental experimental work on the relationship between damping capacity and structure. We also agree that the machine in its present form is not suited to quick non-destructive testing, an aspect dealt with more fully in our reply to the comments submitted by Messrs. Savage and Thring.

It is both interesting and gratifying to learn from Messrs. Savage and Thring that B.I.S.R.A. propose making use of the apparatus we have developed for tests on ferrous materials. The damping capacity of these materials will in most cases be substantially greater than that used in the development of our apparatus, and some further work will certainly be needed before satisfactory results are obtained. We do not regard the improved large machine as at all suitable as a basis of design for a new instrument, and as we mentioned when presenting the paper, the small machine will require further development before it can be applied conveniently to the testing of ferrous materials.

The study of diffusion processes, of the type considered by Snoek, Polder, West, and Kê, raises the question of damping capacity measurements at low frequency and high temperature. The small machine in its present form cannot be used below 150 cycles/min. since, as the specimen diameter is reduced, the danger of elastic instability becomes increasingly great. This difficulty may be overcome by inverting the machine. Much further work will be needed before the small machine could be modified for work at elevated temperatures. The problems are associated principally with the gripping of the specimen, and there is no doubt that apparatus of the type developed by Hanstock and Murray offers distinct advantages in this direction, since the necessity for gripping is eliminated. It is of interest to note in this connection that Hanstock found it advisable to shrink sleeves of higher electrical conductivity on to the shanks of ferrous specimens; our experience with the small-machine specimens indicates that, if the mating surfaces are slightly tapered, losses from interfacial movement are unlikely when the sleeve is shrunk on. Severe chilling of the specimen for this purpose appears inadvisable in the case of steels in which austenite may be retained at room temperatures.

There is no doubt that in apparatus of the form developed by Kê extraneous losses are not inconsiderable, but for the particular application for which the apparatus was designed, the high damping capacities (greater than 50% in some cases) rendered such losses relatively unimportant from a qualitative point of view. The exact order of the temperature-dependent apparatus loss can be assessed only by direct experiment.

The suggestion made by Messrs. Savage and Thring on the relationship between damping capacity and the absorption of supersonic waves is interesting. In that damping-capacity tests at high frequencies generally measure the attenuation of a standing wave system, the forces resulting in the attenuation of supersonic waves propagated through a material could therefore be derived from measurements of this kind. The experimental difficulties are likely to be increased by scattering of the supersonic radiation at discontinuities in the material, for example, at grain and phase boundaries, and, in addition, as a result of different velocities of propagation in different constituents along different crystallographic directions.

In conclusion, the authors wish to thank all the contributors to the discussion for the encouraging and kind way in which they have received the paper.

CORRESPONDENCE ON PAPER BY MR. E. C. ELLWOOD AND MISS JEANNE M. SILCOCK:
 "THE LATTICE SPACINGS OF THE SOLID SOLUTION OF COPPER IN ALUMINIUM."

(*J. Inst. Metals*, this volume, p. 457.)

DR. H. K. HARDY,* M.Sc., A.R.S.M., A.I.M. (Member), and MR. T. J. HEAL,† B.Sc., A.Inst.P.: There are two points in this paper which we wish to discuss. The first concerns the temperature measurements and the second the lattice-spacing values reported by the authors.

We can replace the millivolt readings in Table II, obtained when the authors were calibrating their thermocouple, with temperatures taken from a standard table for Pt-10% Rh thermocouples.‡ Table II then shows the results given below in Table A.

TABLE A.—*Details of the Calibration of the Thermocouple.*

Metal.	Melting Point, °C.	Original Calibration in Molten Metal, °C.	Original Calibration in Camera, °C.
Zinc	419.5°	417.7°	420.9° → 421.9°
Aluminium-Copper Eutectic .	548.0°	543.9°	552.4° → 553.4°
Aluminium	660°	655.6°	663.8° → 664.7°

It will be seen that the authors' two sets of values fall equally on either side of the true melting points. Platinum-rhodium thermocouple wire is normally guaranteed to $\pm 1^\circ$ C. of the standard curve. It seems worthwhile, when considering the discrepancy, that the authors should bear in mind the possibility that part of it may be due to the original calibration in molten metal having given somewhat low results.

Turning now to the lattice-spacing measurements, the authors estimate their over-all error as ± 0.0003 kX. units. They also state that their result for the lattice spacing of aluminium confirms the value obtained by Wilson.§

The authors' result for aluminium at 18° C. is given as 4.0491 kX. units; we have corrected this to 4.0497 kX. units at 25° C., in order to provide a direct comparison with figures given by Wilson and other workers, their results being given in Table B.

The last three values agree with unpublished figures of our own, namely 4.0413, kX. units at 25° C., and all are comparable in that none has been corrected for refractive index. Thus, the authors' results are some 0.008 kX. units higher than those of other workers.

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‡ W. F. Roeser and H. T. Wensel, *Temperature: Its Measurement and Control in Science and Industry*, 1941, p. 1294 (Table I) (Reinhold).

§ A. J. C. Wilson, *Proc. Phys. Soc.*, 1941, 53, 235; 1942, 54, 487.

TABLE B.

Reference.	Lattice Spacing of Super-Purity Aluminium.	Temp., ° C.
Ellwood and Silcock	4.0497 kX. units	25°
Wilson *	4.0413 ₄ crystal Angstroms	25°
Jette and Foote †	4.0413 ₆	25°
Axon and Hume-Rothery ‡	4.0413 ₄ kX. units	25°

* *Loc. cit.*

† E. R. Jette and F. Foote, *J. Chem. Physics*, 1935, 3, 605.

‡ H. J. Axon and W. Hume-Rothery, *Proc. Roy. Soc.*, 1948, [A], 193, 1.

This discrepancy is about 25–30 times the authors' estimated error, and strongly suggests that there may be a further unrecognized error in their work.

As an indication of where this error may lie, we see that the authors appear to have confused kX. units, crystal Angstrom units, and absolute Angstrom units. Thus, they quote Hume-Rothery § for the lattice spacing of copper, as 3.6147 kX. units. Hume-Rothery and Andrews's figure in the paper quoted is 3.6074 crystal Angstrom units, which is, of course, 3.6074 kX. units.

The appropriate conversion factor, as given by Bragg, || is 1.0020₂ for conversion from kX. units to Angstroms, i.e. 1000 kX. units = 1002.02 absolute Angstrom units. The factor for the reverse process is the reciprocal of this, and if Hume-Rothery and Andrews's figure for copper had, in fact, been in absolute Angstroms, the appropriate kX. value would have been obtained by dividing by 1.00202, not multiplying. Thus, in this case the authors have applied an unnecessary correction and in any event have applied it wrongly. It appears possible that the measured parameter values have been subjected to a similar error, in which case the values given in the paper should be read as absolute Angstrom units and not as kX. units as stated.

A case such as this stresses the desirability of providing all the relevant constants on which parameter values are based in any paper on the subject, a point of view which Bragg || has expressed as Chairman of the X-Ray Analysis Group of the Institute of Physics.

The AUTHORS (*in reply*): We wish to thank Dr. Hardy and Mr. Heal for their comments on our paper. With regard to temperature measurement it had not occurred to us to compare our calibration figures with the standard table for Pt–10% Rh thermocouples, and while such a comparison appears to present the results in a somewhat better light, we do not think that such a comparison is really justified, since the same thermocouple, circuit, and potentiometer were used for the calibration in molten metal and for the calibration in the camera, and it is unlikely that the molten metals used for calibration, which included lead and tin as well as those recorded, should have been contaminated so as to give a systematic error. Agreement within 3° C. between the two methods of calibration has recently been obtained at 660° C., since the potentiometer has been sent for overhaul and checking and a 0.15-mm.-dia. thermocouple used instead of the 0.4-mm. thermocouple which was in use when this work was carried out. We are not entirely satisfied, however, and experiments with a composite sample comprising silver and the alloy under investigation are being carried out with a view to using the lattice spacing of silver (after suitable calibration) as a measure of temperature.

With regard to the lattice-spacing measurements, Dr. Hardy and Mr. Heal have revealed an unfortunate error for which only one of us (E. C. E.) is respon-

§ W. Hume-Rothery and K. W. Andrews, *J. Inst. Metals*, 1942, 68, 19.

|| W. L. Bragg, *J. Sci. Instruments*, 1947, 24, 77.

TABLE C.

[illegible]

sible. The original measurements were recorded as crystal Angstroms, but on return from the Institute's referees these figures were quite wrongly multiplied by the factor 1.00202. We apologize most sincerely for committing this error.

The original values obtained at elevated temperatures, in kX. units, are shown in Table C.

At 18° C. the values for the lattice spacing of the quenched samples were as follows: pure Al, 4.0409 kX. units; 0.95% Cu, 4.0390 and 4.0389 kX. units; 1.95% Cu, 4.0369 and 4.0367 kX. units; 2.98% Cu, 4.0349 kX. units; 3.96% Cu, 4.0328 kX. units; 4.97% Cu, 4.0306 kX. units. The figure for copper taken from Hume-Rothery and Andrews's paper should read 3.6074 kX. units.

The results shown in Table III, and Figs. 4 and 5 of our paper are not materially affected by these alterations.

DISCUSSION ON PAPER BY MR. E. WILFRED TAYLOR: "MICRO-HARDNESS TESTING OF METALS." *

(*J. Inst. Metals*, this volume, p. 493.)

PROFESSOR H. O'NEILL,† M.Met., D.Sc. (Member of Council): I greatly welcome this paper, because in addition to eight foreign instruments (Bierbaum, Knoop (Tukon), Lips, Woxen, Bergsman, Eberbach, Zeiss-Hanemann, and Girschig), we now have evidence of the production of a British hardness micro-tester. I hope that the new instrument will quickly get through any teething troubles and also that the term "micro-hardness" will be dropped.

Hardness micro-testing appears to be of academic and industrial value for studying (a) brittle and hard substances, (b) thin substances like foils, electro-deposits, and coatings, (c) various constituents in a microstructure, and (d) orientation effects in single crystals. In connection with this last subject, exactly twenty years ago I published ‡ work carried out with a home-made micro-tester, which used loads of 50 g. and upwards on a hemispherical diamond indenter. The irregular-shaped impressions obtained on single-crystal grains, as previously observed by Osmond and Cartaud in 1906, led me to use the instrument for ruling scratches in pre-determined crystallographic directions. In this way one evaluated the resistance to scratching along various crystal axes. The author's remarks on p. 499 in relation to Fig. 12 bring us right up against this subject. The pyramid micro-indentations on the crystal of phosphor bronze have their diagonals orientated in various directions, and the hardness numbers are said to vary from 129 to 183. What was the hardness value of this crystal? The matter cannot be left like that, and we must probably have a code of practice for hardness micro-testing on single grains.

Testing of the various constituents of a microstructure is a fascinating and valuable procedure, and in 1934 I put before the Institute some results obtained with a 120° diamond cone, concerning the constituents in bearing metals.§ The period of loading on the constituents was continued for measured lengths of time to determine the flow properties, and this is a further point which I would recommend to students of the subject. It is rather surprising that on p. 498 the author reports the same value of 200 kg./mm.² for both ferrite and pearlite in a medium-carbon steel. Using a Lips-type micro-indenter made in the L.M.S. Research Laboratory to work at the end of a microscope tube with a spring load of 48 g., I obtained || micro-pyramid values of 90 kg./mm.² for Hilger pure iron and 113 kg./mm.² for commercial ingot iron. These are much lower than the author's values for iron, and after reviewing the whole subject ¶ it appeared that representative numbers for ferrite and normal pearlite were 85 and 200, with 750 for iron carbide (as compared with the author's 612). There seems to be something slightly wrong somewhere, and perhaps the clue is to be found in Figs. 3, 4, and 17. The pyramid indentations do not appear as squares in plan, and the author will probably agree that the indenting system is not quite axial. We learn on pp. 495 and 496 that

* Discussion at the Annual Autumn Meeting, Cambridge, 15 September 1948.

† Professor of Metallurgy, University College, Swansea.

‡ *Carnegie Schol. Mem. Iron Steel Inst.*, 1928, 17, 109.

§ *J. Inst. Metals*, 1934, 55, 51.

|| *Trans. Inst. Weld.*, 1946, 9, 6.

¶ *Metallurgia*, 1941, 23, 71.

the lower indenting loads must not be removed by reversing the slow-motion mechanism, and are instructed instead to pull the chain marked *S*. That does not sound too good, and the author should be encouraged to put the finishing touches to the mechanism and provide us with a British machine that is second to none.

A Sub-Committee of the British Iron and Steel Research Association is studying micro-indentation methods and is aware of the many difficulties which accompany their use. The danger of overloading from inertia effects is serious, and friction can also introduce errors. The instrument made by Girschig and described in a remarkable paper,[†] tries to overcome both these effects and permits tests to be made on electrodeposits of copper only 15 μ thick.

Metallic manganese at room temperature is a very hard and brittle metal which I have lately been studying. Table A shows some values obtained on a sample kindly provided by Dr. Allen of the National Physical Laboratory. The Tukon tests were made on what appeared to be two different phases, *A* and *B*, in the specimen, and these micro-indenters enable one now to say that manganese has a pyramid value of about 900 kg./mm.² and is the hardest of the metals.

TABLE A.

	Vickers Diamond Test.		Zeiss-Hanemann Test (by courtesy of the Bragg Lab.).		Tukon Machine (by courtesy of the A.E.R.E., Harwell).		
	Load, kg.	Hardness.	Load, g.	Hardness.	Load, g.	Vickers Hardness.	Tukon Hardness.
Manganese	1	839 *	41	1675	<i>A</i> 50	754	926
		958 *	52	1400		618	918
		916 *	75	1465	<i>A</i> 100	705	898
		958 *	100	1345		737	880
		916 *			
	5	1027 †	<i>B</i> 100	1049	...
		1027 †		957	...
		1072 †
Hilger pure iron		...	41	104
		...	52	100
		...	75	96
		...	100	91

* Metal cracked slightly.

† Cracked and splintered.

It will be observed that the hardness values increase progressively at the lower loads, and it is general experience that the law of geometrical similarity does not hold for these extremely small indentations. The cause of the deviation is perplexing. Surface hardening due to plastic flow looks a feasible explanation, for the preparation of the specimens is one of the great worries about this type of testing. Various experiments have satisfied Bergsman that surface strain-hardening is not the source of the trouble. If this is true we are left with the possibility of effects due to elastic recovery or creep or even surface tension of the solid metal. The latter is unlikely, for published macro-indentation tests on copper using conical indenters have shown a decrease of

† *Rev. Mét.*, 1946, 43, 95.

hardness value at the higher testing loads, and so the trouble is not apparently due entirely to surface effects. As creep is not likely to take place in hard manganese, elastic recovery is perhaps the cause of the trouble.

Further work is now being carried out to try to clear up these unusual features, and we look forward with every good wish to the author's instrument being used in such researches.

MR. L. ROTHERHAM,* M.Sc., F.Inst.P. (Member): I have a rather different point of view from that of Professor O'Neill, but in part at any rate the conclusions which I reach are the same. My interest in hardness testing arises from industrial work. Some years ago I was concerned with work on the development of the Firth Hardometer, which is a spring-loading machine now giving a range of loads from 2 to 120 kg. The author of the present paper regards a spring-loading device as unreliable, and this may well be the case for very low loads, but is not, of course, generally true. The spring-loading machine which Professor O'Neill described is one which we did work with, or at any rate we worked with one built on the same principles but with, I am afraid, a good deal less care. Difficulty is experienced unless the utmost degree of care is taken with a spring-loading machine at very low loads. My own view is that micro-hardness testing is a highly specialized procedure, and should not be regarded as an addition to ordinary hardness testing but as a technique in its own right. This implies that the operator should know what he is doing, and in any case he is required to be able to maintain a projection microscope in good condition. Micro-hardness testing will generally be applied to research problems, and a suitable operator might be expected to maintain a spring-loading machine in perfect condition, in which case it should give reliable service.

The direct-loading device, however, has many advantages, and as far as I can see it has only two real disadvantages, the higher inertia of the system and the friction in the bearings. As far as the inertia is concerned, obviously the load on the indenter must be increased by the fact that a definite force is needed to raise the lever off its contact. The indication of Fig. 5 is that the effect is completely unimportant at loads above 25 g. At lower loads some uncertainty is introduced owing to the difficulty of measuring the very small impression, and I wonder whether the author, in what has obviously been a very careful study, has noticed any effect at lower loads. Inertia troubles could be eliminated by the introduction of a device such as the soft spring or sorbo rubber used to support the loading lever in the Vickers hardness machine, and this might be desirable to avoid errors due to clumsy handling.

In the older Vickers hardness machines, friction in the ball-bearings was common, even at loads as low as 5 kg., but I have never detected the same effect in the newer machines using knife-edge bearings, even with loads as low as 1 kg. With careful maintenance, a good bearing would probably be quite satisfactory.

I may be appearing to make suggestions which are critical of the design of the present machine, but I should not like to leave that impression. What I am presenting is simply an alternative point of view. I shall be very glad to obtain a machine made by the author for use in my own laboratory.

I should like to raise one point of general interest in connection with Fig. 5. I take this material to be the ordinary cutlery type of stainless steel. This is one of the most difficult materials to polish, and it is almost impossible to polish it without some softening of the surface layers. The effect of softening of the surface layers seems to me to be somewhat more pronounced when dealing with the softer rather than the harder grade of the material. For example,

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with fully tempered material with a hardness of about 200 Brinell I found that we could hardly polish it at all if we were to avoid softening of the surface layers. Therefore I am interested to know how the polishing was done on this particular sample, and I should like to ask whether in fact it has been polished by mechanical means, or by electrolytic polishing, or something of that sort.

I agree with Professor O'Neill that this is a machine which has been awaited by metallurgists in this country for a long time. It is very annoying if one wants just half a dozen micro-hardness tests to have to make one's own machine to do them.

DR. W. HUME-ROTHERY,* M.A., F.R.S. (Member): Everybody will wish to join with Professor O'Neill in saying how much this sort of instrument is to be welcomed, and how very fortunate we are that it should be discussed before the Institute here. I have often felt that it is unfortunate that in England so many of the instruments are discussed in other journals and not by the metallurgical societies, the members of which are going to use them.

The whole method is one which is very attractive to the University research worker and it seems to offer a wide range of application, but I think that I agree with Professor O'Neill that, if we are going to use it, we must consider very carefully exactly what we mean by hardness.

If we take an ordinary hardness test on a relatively large specimen, in which the area of the specimen is large compared with the crystal structure, then, as explained by Professor O'Neill, we have to impose conditions with regard to the maximum size of the impression which we can allow and how far it must be from the edge of the specimen, the minimum distance allowable between two indentations, and so on; and even when we do that we come to the conclusion, I think, that hardness is a profoundly unsatisfactory quantity.

Some time ago, Professor O'Neill came to examine one of my men who was rash enough to do hardness work, and we discussed these various methods and decided that in some of the work on age-hardening, for example, it was possible to have two specimens and find, if using one indentation instrument, that one specimen would be harder than the other, while if using another instrument the order would be reversed. Hardness is an unsatisfactory property.

When one is dealing with the ordinary test, however, one can at least ignore the effect of individual crystals, but when one is dealing with this instrument and there are crystals of different orientations, then when one gets a single impression on a crystal the hardness has to be defined more carefully. If one defines hardness as resistance to indentation and uses a square indenter, the hardness will have to include the orientation of the indenter relative to the crystal axes. I think, therefore, that the author's conclusion with regard to Fig. 12 of the paper is rather doubtful. He shows there a very careful experiment in which on one crystal he has made a series of indentations, carefully rotating the indenter between each indentation. There appears a series of indentations of different sizes, and the author says that that particular crystal varies in hardness. I suggest, however, that this is a doubtful conclusion; it may well be that the hardness is the same and that the effect is simply the orientation effect.

Suppose one were to consider the relative hardness of two crystals separated by a boundary. It seems that if one makes an indentation which is near to the boundary of the two crystals one may well expect that the exact value will depend on the relative orientations. Great care is needed in the interpretation of the results, and there is a fine field of research in determining the extent to which this instrument can be of use. I myself and others at Oxford have been attracted by it by reason of its possibilities in

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connection with equilibrium-diagram work, where we have increasing difficulty with alloys where it is not possible to identify phases by etching methods, both in metallic compounds of complex aluminium alloys and also in some of the transition elements which are very resistant to etching. We have been attracted by this method for distinguishing between different phases, and also between order-disorder effects. I think that the instrument can be of great value there, provided that one makes a full examination of each individual phase first of all. If it can be shown that the difference between the apparent hardnesses of phases is outside the limits of the variation due to orientation alone, one has a valuable method of distinguishing between phases. Otherwise I think that the method really requires further investigation, and that some of the author's conclusions go rather in advance of what is justified. I am sure, however, that the results obtained with this instrument are very encouraging, and we should all like to see it examined further.

DR. T. B. CROW,* M.Sc. (Member) : As the fourth speaker in this discussion, I am aware that many of the points which I want to raise have already been mentioned, but I am pleased that my opinions for the most part agree with those which have already been stated.

It was with great pleasure that I saw that this paper was to be discussed at this meeting, because in it the author has very ably demonstrated that this instrument has a very wide and, in fact, almost unlimited application in its own field, both in research and in routine hardness testing, provided that the routine hardness testing is done by the proper person.

Most metallurgists have at some time or other been at a loss for some particular piece of equipment for use in their own work, and it may well be that some of us here to-day will readily take up the new technique and the new instrument which the author has made available. I wish that this technique had been available to me some twenty years ago. I was then endeavouring to establish the identity of the interfacial alloys formed when copper was soft soldered. I examined soft-soldered joints from which the excess of solder had been squeezed out by applied pressure. The gap between the faces of the copper was $10\ \mu$ and it was filled with a copper-tin alloy, the micro-hardness of which could have been determined and compared with that of a known alloy. Actually I was able to use other methods, namely scratch-hardness tests similar to those which the author has mentioned in the present paper.

In his paper, the author has concentrated on micro-hardness in relation to microstructure, but there are other equally fascinating applications of the device, in some of which I am interested, namely the hardness testing of cold-rolled metals of a thickness of 0.006 in. and less, as a routine production-control measure. Then there are applications concerned with intrinsic research into some of the problems of cold-rolling metal. As an example of the latter, the existence or non-existence of what is popularly called skin hardness might be established. There are many other matters under both these headings which are of particular interest to me, but time prevents any mention of them now. The Institute may hear something of them later on.

In a paper which J. F. Hinsley and I published in the *Journal* of this Institute two years ago,† we drew attention to certain abnormalities in the shape of the indentations in ordinary Vickers hardness testing which were associated with the orientation of the impression with respect to the direction of heavy cold rolling. It is interesting that the present author has noticed the same kind of thing with micro-tests at different orientations on single crystals. Hinsley and I observed distinct differences in the shape of the impressions,

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† *J. Inst. Metals*, 1946, 72, 461.

caused by directional effects due to cold rolling, but Mr. Taylor and also the last speaker have referred to indentation-shape anomalies on single crystals as related to lattice structure. It would seem obvious that micro-indentation shape must be influenced by lattice orientation, and it may be that some ruling in this respect will be necessary for making micro-tests, just as Hinsley and I advocated in Vickers testing on cold-rolled strip.

The author has drawn attention to the layer of work-hardened material which might remain on a specimen which has not been properly prepared. The importance of this in hardness micro-testing may be gathered from some figures given by Dance and Norris.* They state that abrasion in preparing a specimen caused an increase in hardness—this was in a chromium-molybdenum steel—to a depth of from $3\frac{1}{2}$ to $6\ \mu$, according to the degree of abrasion. Since with a standard diamond indenter the depth of the impression is about one-seventh of the length of the diagonal, and the diagonal itself may be as small as 4 or $5\ \mu$, it is clear that in such a case the hardness number reported would be that of the abraded layer and not of the metal beneath; and therefore one of the things which must be borne in mind in these hardness micro-tests is the choice of a suitable load, not only in respect of the area available on which to make the indentation but also in respect of the possible layer of work-hardened material, due to the preparation process.

In the paper there are two specific points which call for an answer. The first is the question of the hardness of ferrite and pearlite. On p. 497 the author gives the hardness of pearlite as 212, and that of ferrite as 266. Professor O'Neill has already mentioned this, and no doubt the author will be able to deal with it in his reply. I thought that perhaps the values had been accidentally interchanged in the printing of the paper.

As Professor O'Neill has also mentioned, in Fig. 12 the question of the hardness of the phosphor bronze requires some little explanation. I took it that the dark area and the light area on which the tests were made were two adjacent crystals. I may be wrong there, but, if I am right, how does it come about that we get a duplex structure, two constituents, in a bronze of the composition which the author gives? It would be assumed that that composition would give an α structure unless the metal were in the cast state; but in that case I should have expected a hardness of 80–90, which is considerably below the lower figure given by the author.

Previous speakers have referred to some of the difficulties in carrying out these hardness micro-tests. There are difficulties in the way, and it may be some time before we get absolute accuracy, or even accuracy of an order equal to that which we get in ordinary Vickers testing; but we must persevere, and I should like to add my tribute of appreciation to the author for putting this machine before us. I think that he has done a very distinguished service to metallurgy, the fruits of which will become more and more apparent as we go on.

Meanwhile, I do not think that any of us need lose heart, either about the possible loss of accuracy or about some of the other difficulties, because there is always, particularly in industry, a field for *comparative* values. In my own particular work, for instance, we often get samples of extremely thin cold-rolled metal—0.005 in. thick, and down to 0.001 in.—coming in from customers, and they ask us to roll them a quantity of the same hardness. It is not possible to test the hardness of these thin samples in the ordinary way, so here is one very obvious application of the instrument; one can get a micro-test on the customer's sample, and then roll samples of metal to different tempers in the thickness ordered and select from these the one which matches up, on a micro-hardness test, with the customer's sample.

* J. B. Dance and D. J. Norris, *Nature*, 1948, **162**, (4106), 71.

DR. J. C. CHASTON,* B.Sc., A.R.S.M., A.Inst.P., F.I.M. (Member): Reference has already been made to the fact that there are in the American literature † some excellent studies of the behaviour of one or two industrial micro-hardness machines, and a study of these gives a clue to some of the machine difficulties which may arise when using a new instrument such as this.

One of them is the possibility of inertia effects. These have been referred to many times this morning, but I am still doubtful whether very great skill may not be needed to avoid inertia effects when the load is applied, as in this machine, by hand. In the American Tukon machine inertia effects were found with the earlier models, and a special solenoid device was added later which was claimed to overcome the difficulty. This is a matter about which I think the user will want a great deal of reassurance; and much experimental work may have to be done before we can be sure of eliminating the effects of inertia.

Another trouble which may give difficulty with a hand-operated machine of this type is vibration, and it may be necessary to take great care in handling to avoid vibrating the set-up and getting erroneous results.

A third defect observed in American machines was a slight rocking of the indenter as the load was applied. In this connection, I have looked at the impressions in Figs. 3 and 4 of the present paper, and it is interesting to see that in these there is a lack of symmetry which is absent from the other impressions illustrated in the paper. I wonder whether these specimens were taken with the same machine as was used to obtain the others, whether the same diamond was used, and, if so, whether there is any reason to believe that it might have been displaced.

A point not raised so far this morning concerns the use of the Knoop indenter, which was introduced by the Bureau of Standards around 1939. It produces an elongated impression, about fifteen times as long as it is wide. It has found a great deal of favour in America, but it is notable that in an American paper ‡ published this year, describing a new micro-hardness machine, the authors have gone back to the Vickers diamond. One reason they give for doing so is that it is easier to read the diagonals. Can anyone confirm whether this is so? Mr. Taylor's idea of using a three-cornered indenter seems to me to deserve careful thought; it is a new suggestion, and it may have many advantages.

Then there is the question of the constancy of the hardness number with different loads. Professor O'Neill has said that with very light loads the hardness number tends to increase, but instances are reported in the American paper ‡ of a decrease in hardness number with very light loads, due apparently to the use of an imperfectly shaped diamond. These American authors point out that with very small loads only the tip of the diamond is used, and if the tip is slightly rounded, erroneous readings will be obtained under light loads. The difficulty of making an accurate diamond may be the governing factor in the choice of the indenter in future, rather than, as in the past, the ease of measuring the impression.

There is some indication in the present paper, in Fig. 5, that Mr. Taylor does not find any difference in hardness down to a 25-g. load, but he has not reported the hardness figures represented by Fig. 4, where impressions with

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† C. B. Brodie, *Trans. Amer. Soc. Metals*, 1944, **33**, 126.

‡ D. R. Tate, *ibid.*, 1945, **35**, 374.

N. W. Thibault and H. L. Nyquist, *ibid.*, 1947, **38**, 271.

L. P. Tarasov and N. W. Thibault, *ibid.*, 1947, **38**, 331.

‡ R. F. Campbell, Q. Henderson, and M. R. Donleavy, *Trans. Amer. Soc. Metals*, 1948, **40**, 954.

loads down to 1 g. are illustrated. I do not suggest that if we knew the results with this particular specimen, we could judge the constancy of hardness number with load, because this is a polished austenitic stainless steel and would obviously have suffered considerable surface-hardening in polishing. I mention this to emphasize the importance of methods of surface preparation in micro-hardness testing, and to suggest that in this work it may always be advisable to use electropolished specimens or etched specimens, and not mechanically polished specimens at all.

In introducing his paper, Mr. Taylor disclaimed metallurgical experience but there are one or two metallurgical matters which I hope that he will not mind my mentioning. I am very concerned about Fig. 16, because I cannot think that that is a hard-rolled brass; I should have thought that it was annealed.

Then the specimen illustrated in Fig. 12 is said to be annealed phosphor bronze, but I am very puzzled about the curved lines visible in the micro-structure. I am doubtful whether that specimen was really tested in the annealed condition, and I think that it has had a certain amount of deformation, after annealing. Perhaps the variations in shape of the six impressions may be in part due to an irregular distribution of cold work.

DR. H. K. HARDY,* M.Sc., A.R.S.M., A.I.M. (Member): My position is that of a user of micro-hardness test results. Our particular problem is to determine the hardness of very small components which are made from hypodermic tubing. Our limitation is that they are very hard and we can use only a very light load. That means that the impressions are very small, and any slight variation in the diagonal causes a large variation in the actual hardness value. We have a further limitation in that we can get only two or three hardness values on each polished section.

We have heard a number of warnings this morning about the effect of crystal orientation on the hardness values. But we have not heard very much about the absolute reproducibility of micro-hardness values when carried out with the same relative orientations. There is, in effect, quite a lot of variation between different hardness values. For instance, if one looks at Fig. 3 in the present paper, one finds a large number of hardness values of austenitic steel, and in the last row but one from the bottom the central impression is considerably larger than the other impressions. Could we have the hardness values for at least the two bottom rows?

It is possible that very careful standardization of the optical and working conditions might aid this question of reproducibility, but some investigators have taken as many as 20 impressions with two separate observers before quoting a hardness figure.† The Knoop impression shows elastic recovery, and Dr. Chaston has already referred to the visibility effect.‡ I have examined both Knoop and Vickers impressions under the microscope, and it is very difficult to fix the end of the Knoop impression; it tails off almost to nothing. At very light loads the Vickers impression can also give some trouble. One can get a raised edge immediately round the Vickers impression, and it is difficult to know where to take the exact end of the diagonal, although it is much easier than with the Knoop impression. It is important, from the point of view of the industrial user, that reproducibility should be as high as possible. Anything which can be done to increase this will be very welcome to those who want to take only a few impressions in a limited amount of time and get

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† H. Bückle, *Metallforschung*, 1946, 1, 43.

‡ D. R. Tate, *Trans. Amer. Soc. Metals*, 1945, 35, 374.

N. W. Thibault and H. L. Nyquist, *ibid.*, 1947, 38, 271.

results which can be compared among themselves, even if they are not necessarily absolute hardness values.

The surface preparation is undoubtedly very important, and anything that we can be told about the effect of polishing or the effect of different degrees of etching on the hardness values will be very valuable indeed.

My only other point is on Fig. 17: could we have the actual hardness values for the specimens? It is said in the paper that they are in good agreement, but I should like to have the actual hardness values. There are no photographs in the paper of the impressions at a high magnification. In order to measure very small impressions, one has to go up to at least 1000 dia., and preferably 1500 dia., and there one gets very particularly this difficulty in determining the exact end-point to the diagonal. Anything that the author has to say about this will be very welcome.

DR. E. OROWAN,* F.R.S. (Member): Professor O'Neill and other speakers have mentioned that hardness values seem to have a tendency to increase as the indenting load, and thus the size of the indentation, decreases below a certain limit. This may be due to specific causes arising from individual properties of the specimen (e.g. to the presence of a hard surface layer due to mechanical polishing); it is worth pointing out, however, that such a behaviour ought to be expected generally for a fundamental reason, even if specific causes are absent.

Sir Lawrence Bragg has propounded a general theorem† according to which the yield stress is bound to increase above its normal (macroscopic) value if the area to which glide is confined becomes too small. To see why this must be so, let us consider the case where glide is confined to a square of edge length t in the glide plane. In Fig. A, the glide plane $g-g$ is perpendicular to the plane of the drawing, and $s-s$ is the side of the square. As a consequence of the plastic glide, elastic strain is released in the surroundings of the square; for a rough order-of-magnitude estimate we can assume that the elastic strain changes homogeneously in the cube $abcds$ which is halved by the glide plane $g-g$, and that there are no changes of elastic strain outside the cube. If the elastic shear strain in the glide plane, parallel to the glide direction, was γ before glide, a line $x-x$ that was normal to the glide plane in the unstrained material makes an angle $\alpha \approx \gamma$ with the normal after straining but before glide. When glide occurs, the two halves of the line $x-x$ swivel around the points x into positions $x-y$ where the angle of shear α is smaller; the corresponding amount of glide displacement is $y-y$. The maximum glide displacement for which the elastic shear strain in the cube decreases is of the order of $2 \cdot \frac{t}{2} \tan \alpha = t \cdot \gamma$, where t is the side length $s-s$ of the square. If plastic glide takes place over the area of the square, the glide displacement must be at least one interatomic distance a in the glide direction; glide cannot occur unless

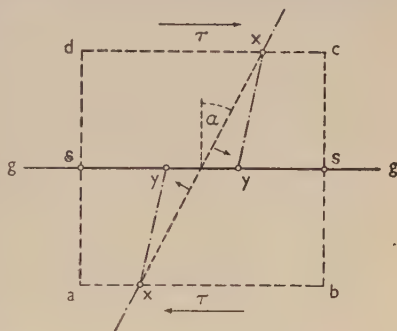


FIG. A.

* Reader in Metal Physics, Cambridge University.

† Sir Lawrence Bragg, *Nature*, 1942, 149, 511. The theorem was used in a special case by E. Orowan, *Nature*, 1941, 147, 452.

the maximum possible glide displacement that can take place under release of elastic strain exceeds one interatomic spacing. The condition for plastic glide to occur within a square of side length t , therefore, is

$$a < t\gamma$$

or, if Hooke's law for elastic shear, $\tau = G\gamma$, is used,

$$\tau > G \frac{a}{t}$$

where τ is the applied shear stress needed for producing plastic glide within the square of side t , and G the shear modulus.

For pure metals, the initial yield stress is usually between 1/10,000th and 1/1000th of the modulus of elasticity; the above-mentioned condition, therefore, is satisfied so long as t is greater than about 1000–10,000 interatomic distances, or about 0.3–3 μ . If t is smaller, higher shear stresses are needed to produce glide by the amount a with a simultaneous decrease of the elastic strain in the surrounding cube. The distribution of plastic deformation around an indentation is very inhomogeneous; we may expect, however, that the effective yield stresses in the plastically deformed region will not be raised noticeably above the macroscopic values if the diameter of the indentation is at least 5 or 10 times the limiting value of t , i.e. at least 1.5–30 μ , according to the ratio between the ordinary yield stress and the elastic modulus. Since the smallest indentations shown in Mr. Taylor's photographs have an edge length of about 2 μ , the corresponding hardness values must be expected to be higher than those obtained with large indentations.

The AUTHOR (*in reply*): There are some remarks which I think are worth making at this moment. First of all, what is the hardness figure of a crystal? I do not know the answer to that, and perhaps I ought to have been more cautious on p. 499, where I say that the area is not of uniform hardness. It might have been more correct to say that the figures derived were not uniform; but it is quite certain that, when one does use the diamond indenter on these crystal forms, the orientation of the crystal and of the crystal lattice exercises an influence, and one gets these impressions with concave or convex boundaries. In fact, instead of getting a square impression it may in certain circumstances become almost circular. What I observed, however, is there to be seen by anybody. It may be that a hardness figure should be an average of a number of impressions taken at different orientations, but the departures from the average are also significant.

As regards flow tests and creep properties; with some materials it is possible to see a very beautiful ripple effect beyond the diamond impression, so that a section across shows a series of waves; this is quite easy to observe. It is also possible, as has been suggested by Professor O'Neill, to study the creep properties, as the size of the impression does change as the loaded diamond remains in contact with the specimen over a period.

Then there is the point about the hardness of ferrite and pearlite, which I knew would come up. I can only say that in attempting to measure the hardness of the latter the plates of pearlite seemed to shatter, and I do not think that one is doing much more than take another measurement of the ferrite. I am sure that there is something there that is well worthy of further study, but on numerous occasions we have obtained the same results, and there is nothing wrong with what I have stated except that I should have said, perhaps, not that they were hardness figures but that they were the figures which were obtained by this method.

Professor O'Neill did not like pulling the chain. There is no need to do so on the instrument which was on view yesterday. The fine motion there is

very perfect in its action, and one can reverse the movement without any evidence of hysteresis of the fine-motion slide; but one must have in mind the fact that one does not know on what machine the hardness tester may be used, and it is therefore necessary to make some provision for the imperfections that usually exist in slow-motion mechanisms.

I am very pleased to find that so far no objection has been raised to the use of a triangular diamond.

We come then to the question of the constancy of the figures obtained at different loads. We stopped at 25 g., and after that the figure went up. I had attributed that to work-hardening of the surface layers of the specimen. Dr. Orowan has given other reasons, and it may be that he has drawn our attention to the real explanation.

Mr. Rotherham referred to spring loading. In the first indenter which we made the specimen was carried on very delicate springs, which seemed most beautifully responsive, but we eventually abandoned it for a gravity system, and the results at once showed much greater consistency. It is possible to study the inertia fairly easily by bringing the diamond up to the specimen at different speeds. We have recommended $10\mu/\text{sec.}$ as a safe speed. That is very easily controlled, and one can soon determine what is a safe speed for a particular specimen.

I was asked whether Fig. 4 was polished mechanically. It was. I believe that we used oxide of cerium as the polishing medium and had some difficulty in getting a good mirror surface.

Dr. Hume-Rothery again raised the question of average hardness. As I have said, as the diamond is rotated, impressions of varying size are obtained, and how to deal with that is a matter for consideration.

Dr. Crow mentioned soft-soldered joints in copper. We did experiment on soldered joints, and in some cases the materials soldered together seemed to diffuse to some extent into the solder, and we got different hardness figures in the centre of the joint and very close to the edge. There would be no difficulty whatever in dealing with cold-rolled metal strip of 0.006 in. thickness and downwards. For instance, the plating shown in Fig. 18 was only about 0.002 in. thick.

The orientation of the impressions in relation to the direction of rolling is, I think, a similar problem to that of measuring the hardness of crystals, since the diamond impression takes a different form in relation to the direction of rolling.

Dr. Chaston referred to inertia effects, and also to vibration. Vibration can be very troublesome. Reference has been made to a bad indent in Fig. 3. At that time a knock had developed in a water-pipe some distance from the machine. It was very intermittent, and we tried to avoid it, but it did coincide with the forming of that impression and caused a slight disturbance just at the moment of contact. I think there is no doubt that an instrument used for this purpose should be on a good foundation, and that it is very important to avoid vibration.

As regards the rocking of the indenter, to which reference was made in connection with Figs. 3 and 4, I should have explained that two diamonds were used, one of which had not been perfectly ground in relation to its axis. I think you will note that the same form of impression as in Fig. 3 is visible in Figs. 17 and 19, where that same diamond was used. I did not take very much notice of that, because I knew that in time we should get more perfect diamonds. Most of the others were taken with the second diamond.

I am afraid I cannot talk about the Knoop indenter, of which I have no experience.

As regards mechanically polished and etched specimens, I cannot claim that we have done sufficient work in that connection really to determine when

it is safe to polish mechanically. I think there is no doubt that where very light loads are to be used the etching process is the safer.

I was asked whether Fig. 16 was really hard-rolled brass. I can only say that it was bought as hard-rolled brass.

Dr. Hardy referred to hypodermic tube. We have had a number of samples sent to us at the works and have tested them for hardness, and no difficulty was experienced. As regards reproducibility of results, leaving out the bad impression in Fig. 3 there is considerable similarity between them, but should perhaps have gone further and at the same load taken 100 impressions, measured them up, and given a figure of probable error from the mean. I will do something of that kind. With regard to the hardness values of Fig. 17, we generally get a variation of something like 6 or 7% over a series of that kind. I do not appear to have given the actual figures in that case.

I feel that my answers have been too brief in many cases, but I am grateful for the interest which has been shown in this paper and for the many points which have been brought forward during the discussion.

CORRESPONDENCE.

MR. F. J. DUNN * (Member): I cannot see what this hardness machine will contribute to present-day industrial research. For some years now, various gadgets have been applied to the microscope of hardness machines, by men employed in industry, for enlarging and projecting metal structures and hardness indentations of metals on to screens to enable several persons to view the enlarged projection at the same time.

I would be interested also to know what difference it makes as far as the hardness of a metal is concerned, whether the diamond is located on the crystal or on the boundary of the crystal. Most fractures in metals occur in the crystal, and not at the grain boundary (whether the fracture is caused by a tensile pull or otherwise). Crystals in metals are considered to be homogeneous in an atomic sense, and cohesion between crystal grains appears to be greater than in the crystal itself.

The diamond supplied with the present type of Vickers hardness machine is known to fracture when testing hardened tool steel. If the diamond is reduced in size the chances of the diamond fracturing will be greater.

The use of light loads on Vickers machines is avoided in some industrial concerns, as it is considered unreliable. Workers studying the age-hardening properties of some high-purity alloys of the Duralumin type believe that the hard and soft spots found in the metal are due to the rejection of solute atoms from supersaturated solid solution. These atoms are considered to move into interstitial lattice spaces, creating vacant spaces which, it is stated, cause a variation in hardness. To ensure that such alloys have been allotted the specified time for ageing, inspection in some cases demands that hardness tests shall be made, with a minimum load of 10 kg.

Further, if the specimen must be etched to show crystal orientation, as far as steels in the hardened state are concerned it might be more reliable to determine the approximate hardness from examination of the microstructure of the steel rather than from measurement of the hardness indentation. I have had experience on aircraft steel parts where the specification called for an oil quench to last for only 5 sec., and then for the hot component to be plunged into a lead tempering bath. The component was afterwards cleaned and its hardness measured on a Vickers machine. A label was attached to the component showing the hardness figure, and the component was then incorporated

* Liverpool.

into a unit and put into a refrigerator for 8 hr. at a temperature of 60° below zero. The unit was then withdrawn from the refrigerator, taken apart, and the component re-examined. It showed a considerable increase in hardness. Further investigation proved that the freezing process had increased the hardness figure.

It appears to me that the hardness machine described by Mr. Taylor will only be useful with metals possessing large crystal grains. Finally, all hardness machines which depend on the use of a microscope to determine hardness figures are more or less useless for determining hardness on curved surfaces.

MR. P. WHITAKER,* A.M.C.T. (Member): Professor O'Neill referred in the oral discussion to some of the tests carried out for the Mechanical Methods Sub-Committee of the New Techniques Committee of B.I.S.R.A. On a micro-indenter developed in the writer's laboratory, and using a selected 136°

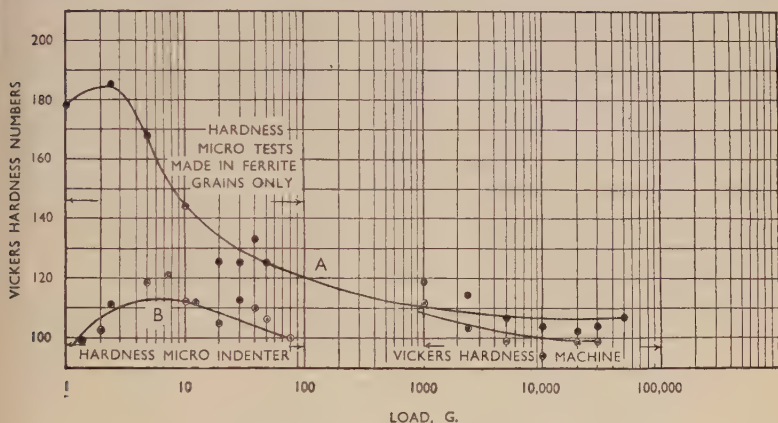


FIG. B.—Vickers Hardness Variation with Different Loads.

- A ●—● Mild steel, normal hand polish, 000 emery, and Selvyt-polished with alumina.
 B ●—● Specimen annealed and electrolytically polished.

diamond pyramid, it has been found that often there is a marked variation of hardness when testing with various loads.

Fig. B shows the results obtained on ordinary mild steel with a normal hand polish, which consisted in emery finishing to 000 grade and finishing with levigated alumina on a rotating Selvyt pad.

These values were compared with results from the same specimen (after the same polishing treatment) which had been afterwards fully annealed at 930° C., furnace-cooled in an inert atmosphere to room temperature, and, finally, electrolytically finished. Examination of these specimens showed that normal hand polishing very seriously disturbs the immediate surface of the specimen, which in consequence gives higher hardness values. The annealing caused a slight lowering of the general hardness of the sample, owing to a small increase in the grain-size.

Fig. C shows the results plotted against the depth of impression, from which it will be seen that the depth of the affected layer is less than 5μ .

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The values obtained on hand-polished H.C. copper are shown in Fig. D (Plate L). This Figure shows that very serious distortion of the surface occurs on normal hand polishing to 0000 emery followed by an electrolytic finish sufficient to remove the 0000 scratches. In this sample, however, the immediate surface layers appear to become much reduced in hardness.

A similar specimen was examined after hand polishing to 0000 emery followed by polishing with metal polish on a rotating Selvyt pad and afterwards annealing to 500° C. in an inert atmosphere. The specimen was cooled fairly quickly by drawing the specimen to a cold part of the furnace; the hardness values obtained on testing with various loads did not show any marked increase at lower loads, and a gradual increase in hardness was observed as the load was reduced. There was, however, no sharp change such as occurred when testing with the hand-polished specimen. Fig. E (Plate L) shows the results plotted against depth of impression. This work shows that

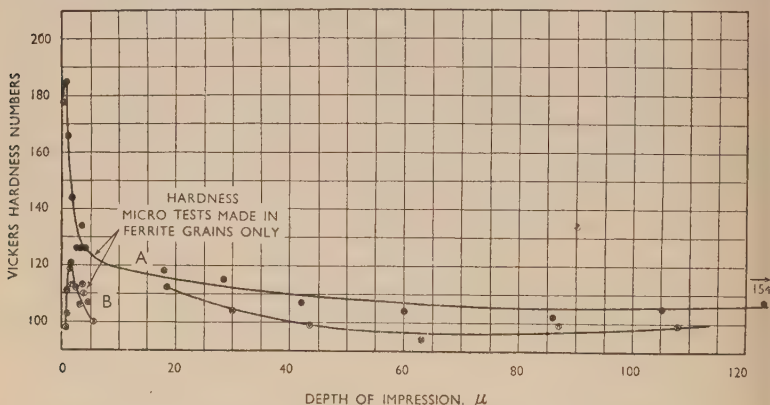


FIG. C.—Vickers Hardness Variation with Different Depths of Impression.

- A ●—● Mild steel, normal hand polish, 000 emery, and Selvyt-polished with alumina.
 B ●—● Specimen annealed and electrolytically polished.

even when careful precautions are taken when using the type of apparatus Mr. Taylor has described, the operator is likely to be very seriously confused if he does not prepare his sample correctly. Moreover, these results confirm the statements of some authorities that in some cases the hardness appears to fall on testing samples with very light loads, whereas others state that the hardness rises on testing with decreasing loads.

The presence of the highly distorted structure on the surface of the copper specimen after hand polishing, represented in Fig. D (A), confirms the findings of previous investigators, and particularly reminds us of the Beilby layer.

Back-reflection X-ray examination of the surface of the specimens showed that the electrolytic polishing has to remove a fair thickness from the surface before it is free from distortion, and in a suitable condition for carrying out hardness micro-tests.

PROFESSOR R. E. DE STRYCKER * (Member): I first wish to congratulate Mr. Taylor on having succeeded in making an instrument that seems to be at the

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same time practical and accurate. Let us hope it will shortly be put on the market at a price which many laboratories will be able to afford.

There is no doubt that many points will be cleared up by the proper use of such a tool. Mr. Taylor has, in his introduction, very modestly waived all claims to being a metallurgist, but I feel it nevertheless necessary to underline that the manner in which he has applied his invention is in several cases incorrect.

It is well known that indentation causes work-hardening at a distance from the impressions themselves; this distance is a function of the shape of the indenter and of the properties of the alloy. A work-hardening effect can sometimes be measured at a distance up to twice the diameter (or the side) of the impression. The indentations on top of Fig. 4, some in the centre of Fig. 10, and most of those on the dark grain of Fig. 12, are much too close to each other.

The difference in hardness obtained after rotating the indenter may, in the case of the light grain of Fig. 12, be due to previous work-hardening (which has well known directional effects) or to the proximity of a grain boundary underneath the polished section. Dr. Hume-Rothery has pointed out that an indentation close to a grain boundary is influenced by the differences in orientation between the lattice of the indented grain and that of its neighbour (when all grains belong to the same phase); therefore most indentations on the right-hand side of Fig. 16 have no practical meaning. Coming back to the light-coloured grain of Fig. 12, it should not be forgotten that a grain boundary may be more or less parallel to the plane of polish, and I feel that this probably accounts for the greater part of the difference between the several hardnesses "measured" on the above-mentioned grain. An invisible grain boundary lies underneath the polished section and is probably closer to the upper angle of the grain than to the lower one.

This same specimen is, furthermore, clearly work-hardened, and there seems to be a single system of glide planes in the light-coloured grain and three systems in the dark one, which may explain the fact that there is a distinct directional effect in the first case and no noticeable difference between indentations in the second.

When a grain is limited by one or more others belonging to a different phase the interpretation of the size of indentations made close to the grain boundary is even more difficult. I would call attention to the indentation on graphite (Fig. 6); it seems to me that in this case nothing of any significance whatever has been measured. This also applies, but to a lesser degree, to the central impression of Fig. 19. It is only in those instances where the impressions are at a distance from the grain boundaries of at least the length of a side of the square, and preferably twice this amount (in order to minimize the effect that underlying boundaries may have), that micro-testing may be considered to have a more or less precise significance. Two impressions should be separated from each other by about twice the side of the square. The micro-test will always be significant if the indentation covers a great number of grains, such as is the case in Fig. 10, for some tests of Fig. 7, for the larger ones of Fig. 17, and the one on the left of Fig. 19.

Reference has been made by two speakers to the merits of triangular indentations as compared with square pyramids. It cannot be disputed that making a square pyramid of small size is a difficult problem; all difficulties do not disappear, however, when a triangular base is substituted for the square one. I believe that the rather unsatisfactory shape of the indentations of Figs. 3-4 is due more to the lack of symmetry of the pyramid with respect to the axis of the microscope than to the fact that the pyramid has a double apex. It is, of course, difficult to say whether this lack of symmetry is due itself to the fact that the base of the pyramid is not a square but a quadrangle

with converging opposite sides, or to the fact that the axis of the pyramid was not mounted parallel to the axis of the microscope. I favour the second explanation in view of the fact that diamond cutters are capable of much more accurate work than is suggested by the first explanation.

If my assumption is correct, even the indenter used in Figs. 3-4 could have given acceptable impressions when correctly aligned, while a triangular pyramid will also give non-equilateral impressions if not correctly mounted.

There are, on the other hand, objections to a triangular indenter: for a given load the impression must necessarily be deeper for a triangular than for a square pyramid, and the uncertainty brought about by grain boundaries underneath the polished section will be greater. Moreover, a square pyramid will, when correctly mounted, give an indentation that, if not perfectly square, will be a lozenge or a parallelogram. If it is a lozenge, the mean of the diagonals is an acceptable basis for measuring the surface of the indentation, and there will be no difficulty in measuring the diagonals for they will be perpendicular to each other and the cross-wires of the micrometer will be parallel to the diagonals; if it is a parallelogram, the angle between the diagonals will never be very far from 90° , and the mean of the diagonals is again both acceptable and easily measured. In both cases, whenever the indenter is not rotated between measurements the directions of the cross-wires can be chosen once for all, and it is possible to use an index with two positions at 90° clamped to the eye-piece tube and to rotate the eye-piece in such a way that a suitable line traced on it is aligned with one of the two lines on the index in a manner similar to the method applied in some types of Continental hardness-measuring machines.

With a triangular indenter this is no longer possible, for the variations in the direction of the sides of the triangle will always be greater than those of the diagonals of a "square", one cross-wire will always have to be parallel with a side, and no fixed index can be used for orienting the micrometer eye-piece; this, coupled with the necessity of making three measurements instead of two, will render the work more tedious. Furthermore, the lower the symmetry of the indenter, the larger the directional effects, while on the other hand no practical use can be made of these effects for a systematic study of the anisotropy of the material, for the triangle does not lend itself well to such a purpose. An elongated indenter, such as the one mentioned by one of the speakers, seems very much preferable for the study of anisotropy.

In short, the disadvantages of the triangular pyramid are such that even a slightly imperfect quadrangular indenter is to be preferred.

The AUTHOR (*in further, written, reply to the discussion and in reply to correspondence*): The main object of this paper was to describe a new design of micro-hardness tester and to attempt to show that it enabled an impression to be made at a pre-determined point on the specimen under controlled conditions of loading.

No doubt, as Mr. Whitaker has pointed out, the methods used in the preparation of the specimen are important, especially when the superficial layer can be shown to possess properties differing appreciably from those of the remainder of the specimen. Experience will also indicate how nearly impressions may approach grain boundaries and each other without influencing the results, and here Professor de Strycker's helpful contribution should be consulted.

It remains to say a few words regarding the instrument, as certain improvements have been made since the paper was read, and additional experience has been gained.

Although many thousands of impressions have been made with the micro-hardness tester and some of the substances impressed have been of extreme

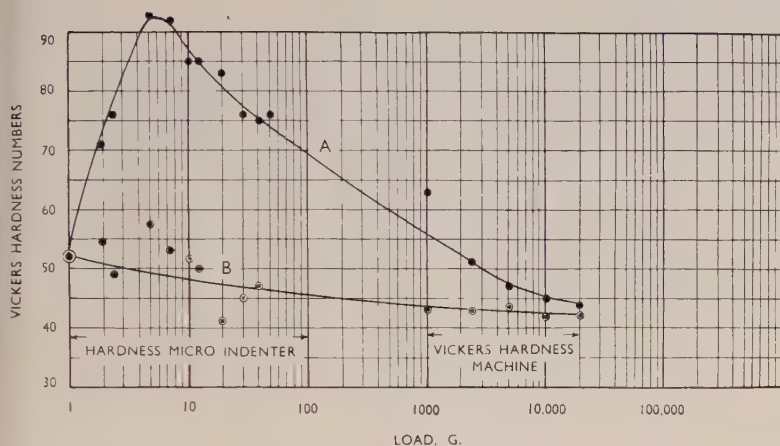


FIG. D.—Vickers Hardness Variation with Different Loads.

- A ●—● Annealed copper, normal hand polish and electrolytic finish.
 B ○—○ Specimen annealed after hand polishing.

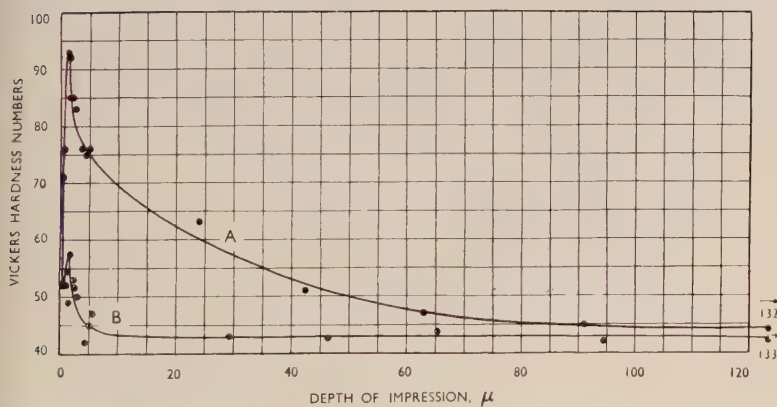


FIG. E.—Vickers Hardness Variation with Different Depths of Impression.

- A ●—● Soft copper, hand polished to 0000 emery, finished electrolytically.
 B ○—○ Specimen annealed after hand polishing.

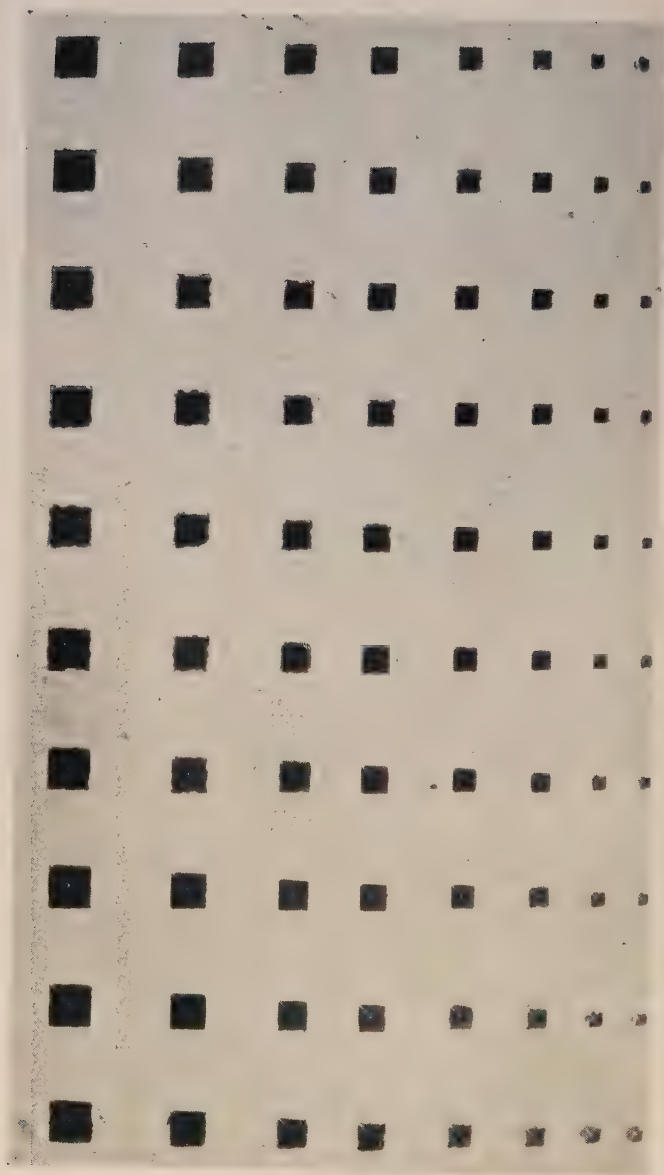


FIG. F.—Impressions Under Loads of 100, 70, 50, 40, 30, 20, 10, and 5 g. Obtained on Ball-Bearing-Quality Stainless Steel (B.S. En. 56).

hardness, such as synthetic sapphire (2670 D.P.N.), we have not yet had the misfortune either to fracture a diamond indenter or noticeably to blunt the point.

It would be very confusing if the diamond pyramid number were found to vary with the applied load, and in our early experiments it was found that the hardness figure decreased with the load; this was thought to be due entirely to a slight movement imparted to the indenter on reversal of the fine-motion mechanism. It became noticeable at loads below 50 g., and a new mechanism was designed. Uniform readings were then obtained down to loads of 20 g., after which the hardness figures showed a tendency to rise. It was then thought that errors in the geometry of the indenter were the cause, and special means were devised to enable a closer approach to be made to the ideal form before the diamond was removed from its holder on the lapping machine, as once registration is lost further correction of the diamond facets is impracticable. The hardness formula assumes the correct form of the indenter.

With a very perfect indenter we have recently obtained the results shown in Table B. Fig. F (Plate LI) shows the impressions obtained with loads of

TABLE B.—*Ball-Bearing-Quality Stainless Steel (B.S. En. 56).*

Loads : 100, 70, 50, 40, 30, 20, 10, and 5 g.

Load, g.	L_1	L_2	L_3	L_4	L_5	L_6	L_7	L_8	L_9	L_{10}	Average, μ	D.P.N.
100	17.5	17.6	17.8	17.4	17.6	17.7	17.4	17.6	17.4	17.5	17.55	602
70	14.6	14.9	14.5	14.6	14.9	14.4	14.6	14.6	14.4	14.8	14.63	606
50	12.0	12.1	12.3	12.6	12.5	12.3	12.3	12.2	12.5	12.7	12.35	608
40	11.1	11.1	11.0	11.2	11.1	11.1	11.1	11.0	10.9	11.2	11.08	604
30	9.6	9.5	9.5	9.6	9.5	9.6	9.5	9.5	9.6	9.5	9.54	611
20	7.8	8.0	7.8	7.9	7.7	7.7	8.1	8.1	7.8	7.8	7.87	599
10	5.3	5.4	5.4	5.6	5.6	5.7	5.7	5.4	5.5	5.4	5.50	613
5	4.20	4.20	4.25	4.20	4.25	4.00	4.25	4.25	4.20	4.30	4.21	523

L_1 to L_{10} represent the mean of the diagonals of each impression measured in microns.

100, 70, 50, 40, 30, 20, 10, and 5 g. The material selected was ball-bearing-quality stainless steel (B.S. En. 56). All the measurements are given, and the departures from the mean reading obtained under a given load are clearly revealed. In Table B, L_1 to L_{10} represent the mean of the two diagonals expressed in microns.

At very light loads, below 10 g., the hardness figures show a strong tendency to fall, and this may be due to shock at impact, since the moving parts must possess mass. It does not appear to be due to the size of the impressions, since the tendency is no less marked when a soft material is tested, but to be related to the load used and the speed of approach of the diamond to the specimen. A similar tendency has been noted elsewhere* and the cause ascribed to imperfections in the geometry of the indenter. It will be clear from Table B that with a load of 5 g. the impressions average only 4.21 μ , or 0.00017 in., and that the diagonals of the impressions must be measured with considerable accuracy.

* R. F. Campbell, Q. Henderson, and M. R. Donleavy, *loc. cit.*

DISCUSSION ON PAPER BY MR. J. S. BOWLES AND DR. W. BOAS: "THE EFFECT OF CRYSTAL ARRANGEMENT ON 'SECONDARY RECRYSTALLIZATION' IN METALS." *

(*J. Inst. Metals*, this volume, p. 501.)

DR. E. OROWAN,† F.R.S. (Member): I think that the best service which I can render in opening the discussion is to spend a few minutes on presenting the subject in a somewhat wider perspective. As you know, we believed not so many years ago that recrystallization was one single physical process, and that the simplest expression of its general law was given in the recrystallization diagram. To-day we know that there are at least four different physical processes which, alone or in combination, determine the shape and size of the grains which arise on annealing in a previously plastically distorted material.

The first is the ordinary or primary recrystallization. If we continue the annealing after primary recrystallization has been completed, we observe a movement of the grain boundaries which is due to the tendency of the material to reduce the total energy of its grain-boundary surfaces. That leads to an increase of the grain-size and to a very characteristic foam cell-like shape of the grains. This process is called grain growth, and what seems to be its fundamental law has recently been discovered by Dr. Cyril S. Smith.‡

The third mechanism is secondary recrystallization. If we severely cold-work aluminium, for instance, and then anneal it, primary recrystallization may be complete within a few seconds. If annealing is continued, a slow movement of the grain boundaries is observed which may last for hours or even for days, not accompanied by any other phenomenon; then suddenly, a number of grains begin to grow very rapidly and consume the primary recrystallized grains just as the primary grains have consumed the grains in the cold-worked material. This second recrystallization without intervening cold work is called secondary recrystallization. It was discovered in 1920 by Masing who also gave the phenomenon its name. It took a long time before its general importance was recognized.

The fourth and most recently discovered mechanism of recrystallization is what we call polygonization. I observed it several years ago on X-ray diagrams, and two or three years ago Mr. Cahn in our Metal Physics Group in the Cavendish Laboratory studied it in single crystals of zinc. Last year Professor Guinier in Paris discovered by means of a very ingenious X-ray method that the sub-grains in recrystallized aluminium observed by Professor Lacombe are the product of polygonization, which seems to be of very much more general importance than was thought initially.

At present, therefore, four distinct mechanisms of recrystallization are known, and among these secondary recrystallization is particularly interesting because we do not know what is its driving force. In primary recrystallization, obviously, the latent energy of cold work is the driving force of the process, but there is no cold working between primary and secondary recrystallization. How can, then, secondary recrystallization take place?

* Discussion at the Annual Autumn Meeting, Cambridge, 16 September 1948.

† Reader in Metal Physics, Cambridge University.

‡ *Metals Technol.*, 1948, 15, (4); *A.I.M.M.E. Tech. Publ. No. 2387*.

There seem to be two views on the origin of secondary recrystallization. Some authors believe that it arises from a high degree of preferred orientation which, as a rule, is associated with a large amount of deformation. They consider that, when the primary recrystallized grains have almost the same orientation, they have a tendency to merge completely, and this would be what is observed in secondary recrystallization.

The second view, represented, for instance, by Professor W. G. Burgers, is that after primary recrystallization the material is not quite free from stresses, and that the residual stresses in the primary recrystallized material are the driving force for the secondary recrystallization. Mr. Bowles and Dr. Boas have carried out a crucial experiment—as crucial as an experiment in metallurgy can be—to decide between the two views. They have produced two sets of cold-worked specimens; in the first, produced by ordinary straight rolling, all lattice directions had a strong preferred orientation, while in the second set, produced by “compression rolling”, only one crystallographic direction was aligned, and the directions normal to this randomly oriented. Both sets were capable of secondary recrystallization, and, consequently, the view that a very similar orientation of all, or most, grains is necessary can be rejected. This, of course, is no proof that the residual-stress hypothesis is correct; there may be other explanations.

The authors stress the point that secondary recrystallization, nevertheless, is much easier if the preferred orientation is more complete. I cannot quite agree with them on this point. Of the three cases of polycrystalline material they have investigated, the fully oriented specimens recrystallized more readily in the case of silver and copper, while the partly oriented (compression rolled) specimens of commercial aluminium recrystallized better than the fully oriented ones. Two favourable cases against one unfavourable case are not sufficient to establish a rule, particularly in view of the circumstance that the two sets of specimens, in spite of the same reduction in thickness, probably did not receive the same amount of cold work. There are two reasons for this. Firstly, different types of preferred orientation are associated with different anisotropies of the yield stress, and we have to expect that the compressive yield stresses of the straight-rolled and the compression-rolled specimens diverged as their preferred orientations developed. For this reason alone, they must have received slightly different amounts of cold work, although it is difficult to say which sign the difference was more likely to have. In addition to this, there is another reason to expect a difference in the amount of cold work received by the two sets of specimens. The type of deformation produced in straight rolling is a plane compression, with the lateral spread confined to one direction (that of rolling); in compression rolling, it is a uni-axial compression with free spread in all directions. The metals used are known to obey the v. Mises-Huber yield criterion rather than the maximum shear stress criterion, and in this case the stress, and with it the work, needed for compression must have been about 15% higher for the straight-rolled specimens. If, therefore, it is the amount of cold work rather than the reduction in height that determines the tendency to secondary recrystallization, the straight-rolled specimens may have been in a more favourable position for recrystallization.

I should like to use this opportunity for a remark about a frequently neglected source of error in work on recrystallization, although it is unlikely to have affected the results seriously in the present case. Plastic deformation in rolling may be very inhomogeneous; the central parts of the strip undergo almost pure compression, while the surface layers may suffer considerable shear in addition to the same amount of compression. A cold-rolled sheet, therefore, is a sandwich whose different layers have received different amount of plastic work. Unless one has particular reasons for believing that the

differences are small, it is very important for thick layers to be etched off both surfaces of the sheet before it is annealed. Only the central layers have received the distortion calculated from the reduction in thickness, and it is not sufficient to remove the surface layers after recrystallization because the nuclei are formed preferentially in the heavily distorted surface layers which, therefore, determine the recrystallized grain-size.

DR. MAURICE COOK,* F.I.M. (Member of Council): In the course of the work described in this very interesting paper the authors have made a number of useful observations which will be helpful in advancing further study of this subject. When I first became seriously and actively interested in recrystallization, grain growth, annealing, and related subjects, which is now nearly thirty years ago, a number of simple generalizations for several materials had been arrived at concerning the effect of such factors as the magnitude of the final rolling reduction and the time and temperature of the final annealing operation on the temperature of recrystallization, or softening, as we spoke of it then, and on the final grain-size. Like all simple generalizations based on long years of practical experience on the one hand, and quite a small amount of critical and exact investigational work on the other, they had their limitations, but, nevertheless, they were of very great practical use and value, and indeed still are to those with a direct interest in the production of strip and sheet materials to grain-size specifications and where grain-size is an important factor in determining the quality and suitability of materials.

In fact, to these simple generalizations little has been added of immediate practical value, even though in recent years a great deal of work had been done on the subject. This, however, has been more concerned with seeking out explanations of the nature of what occurs when deformed or worked metal changes back to the crystalline and undistorted form. While work of this sort is often of a more complex nature than that concerned with establishing the first simple facts and generalizations, it is unfortunately sometimes made unnecessarily complicated and less intelligible and useful to those who might derive some benefit from it, by a regrettable tendency on the part of too many writers to premature theorizing on insufficient evidence and to a confused use of terms and expressions, which is equally unhelpful.

I feel that the time has come, before the confusion becomes too great, to plead for care in the way in which some of these expressions are used, and in the choice of the expressions themselves. Although the use of inverted commas in the title and the wording at the top of p. 502 suggest that the authors have used the term "secondary recrystallization" with some diffidence, I think it is unfortunate that they have used it at all. To quote their own words from the second paragraph of the paper, they use it in reference to grains which grow "on heating *after* recrystallization"—that is, they are really discussing abnormal grain growth and not recrystallization as metallurgists have normally hitherto understood the term. Many other terms and expressions are creeping into writings on this subject, and they are not always used with the same meaning, while some of them seem to have no meaning at all. This is a matter, I suggest, which could be usefully considered by the Nomenclature Committee.

The word recrystallization has always been associated in the minds of metallurgists with the changes in structure and properties which occur when cold-worked or deformed metal is heated under such conditions that the unworked or annealed state is restored, that is, it is thought of essentially in connection with the softening step characteristic of normal so-called annealing curves.

* Director, Imperial Chemical Industries, Ltd., Metals Division, Birmingham.

Some years ago, Macquarie and I* investigated the conditions giving rise to the development of abnormally large crystals in copper sheet, which work the authors appear to have overlooked. We found it possible to produce, in straight-rolled material, very large crystals, that is, several centimetres across, with acicular characteristics, which seem to correspond with those produced by the present authors. I need not go into details regarding the conditions governing their formation, for these are described in the paper, except to say that, in general, relatively heavy final cold-rolling reductions and high final annealing temperatures were necessary, which are the conditions the authors have used in their experiments with copper. There seems little doubt that the formation of large crystals of this type is associated with the single-texture structure. We found that the large acicular crystals (Fig. 1) were, in fact, only developed in material consisting predominantly of a particular structure (Fig. 2), which subsequent work by Richards and myself† showed to be the single-texture.

By cross rolling, what appears to be quite a different type of abnormal grain growth was developed on annealing. These crystals were equi-axed and quite free from the acicular characteristics of the large crystals produced on annealing straight-rolled copper, but the mechanism of their development is not yet completely understood.

It should be noted, however, that other factors not confined to those concerned with rolling and annealing conditions are involved, for it was only with copper of high purity that the effects referred to were obtained. Less-pure coppers subjected to identical processing treatments did not show abnormal grain growth.

Mr. Bowles and Dr. Boas are incorrect in assuming that the large crystals mentioned in the paper by Richards and myself‡ were of the acicular type referred to in their own paper. It is not surprising, therefore, that the orientation which they give for the large acicular crystals they describe is different from that which they quote from the paper by Richards and myself. The crystals mentioned in our paper were nearly equi-axed and had irregular boundaries, being rather similar in appearance to the large crystal shown in Fig. 17 of the paper under discussion. They were developed in copper strip of large initial grain-size, cold rolled with a reduction of about 75%, and finally annealed at 800° C. The orientation of these crystals as reported in our paper was very close to that of one or other of the twin textures in the rolled strip.

The authors have established that the orientation of the large acicular crystals differs from that of the initial single-texture structure in which they grow, the difference, in fact, being equivalent to a rotation of about 30° on octahedral planes. Richards and myself,§ in studying the re-orientation in structure that occurs on annealing heavily rolled brass strip, have shown that it is similar in extent and may be regarded as equivalent to rotation and twinning on certain octahedral planes. These re-orientations of the lattice are considerable, and it seems to us that they are more likely to occur by atomic re-arrangement than by actual rotation of block units.

DR. J. C. CHASTON,|| B.Sc., A.R.S.M., A.Inst.P., F.I.M. (Member): I should like to associate myself with Dr. Cook in deploring the use of "secondary recrystallization" as a new term in this paper; and although I very much

* M. Cook and C. Macquarie, *Trans. Amer. Inst. Min. Met. Eng.*, 1939, **133**, 142.

† M. Cook and T. Ll. Richards, *J. Inst. Metals*, 1940, **66**, 1; 1941, **67**, 203; 1944, **70**, 159.

‡ M. Cook and T. Ll. Richards, *J. Inst. Metals*, 1940, **66**, 1.

§ M. Cook and T. Ll. Richards, *J. Inst. Metals*, 1943, **69**, 351.

|| Chief of Metallurgical Research, Johnson, Matthey and Co., Ltd., Wembley, Middlesex.

admire Dr. Orowan's most lucid exposition, I also feel that secondary recrystallization has not yet been proved to be a new phenomenon. The authors in their present paper, and I think all the speakers, have considered only one line of reasoning on this subject, that of Burgers and Dahl.

There is, however, another suggested explanation for exaggerated grain growth which has been discussed particularly by Bain and other workers on ferrous materials. Their hypothesis to explain "exaggerated grain growth" is that it is associated with the presence of impurities and inclusions in the material. In the case of steel it is found experimentally, according to Bain, that exaggerated grain growth—secondary recrystallization, if you like to call it so—occurs in aluminium-deoxidized steels much more commonly than in steels free from aluminium, and the idea is that the aluminium particles act as blocking agents in recrystallization. All aluminium-deoxidized steels generally have fairly small grain-size; and alumina particles act to stop the grains growing at the normal speed. However, after annealing for a time the resistance to growth may suddenly disappear, and then one gets these large single grains which the present authors ascribe to "secondary recrystallization".

In non-ferrous work a similar phenomenon has been recorded by Beck* with beryllium copper containing additions of 0.3% cobalt as a grain-growth restrainer.

On the general question of this paper, there is another point which I should like to make. The authors do not say anything about the thickness of their specimens. I would refer again here to the same American work, in which it is shown that the thickness of the specimen may have a great effect on the process of recrystallization, and that the existence of the surfaces in a thin specimen may stop grain growth. That is not a criticism of the present paper, but something which needs to be borne in mind in all work on recrystallization.

I should like to ask the authors what they mean by "copper". This follows on the remarks of Dr. Cook, who pointed out that the progress of recrystallization may depend very largely on the presence of impurities. I think that we are entitled to ask for a little more description than the statement that it was copper bar of commercial purity, 99.5%. Was it tough-pitch? Did it have any deoxidizer in it? The same comment applies to silver. To say "silver sheet, 999 fine" sounds very precise, but very small amounts of impurities have effects on recrystallization which are much more than one would imagine. In our laboratory we always like to do a few subsidiary physical tests to see what happens to the silver when it is heated, first in air and then in hydrogen, before we are sure of its purity.

Finally, in connection with Dr. Orowan's remarks, may I ask him whether the applied stresses are 15% greater with straight than with compression rolling when spreading is not restrained by lateral guides. I do not think edge guides were used in rolling the specimens described in this paper.

DR. H. LEPP † (Member): I want to make a comment on recrystallization in general. Recrystallization is not a field in which I have done any systematic work, but there is one point which has always struck me in all studies of this subject.

Recrystallization takes place only after a certain amount of work-hardening, and it is a thermal process. The work-hardening is a mechanical process, and this mechanical energy is sometimes measured and translated into figures giving the percentage reduction, but never into energy units. Then these

* P. A. Beck, J. C. Kremer, L. J. Demer, and M. L. Holzworth, *Metals Technol.*, 1947, 14, (6); *A.I.M.M.E. Tech. Publ.* No. 2280.

† Consultant, Compagnie Française des Métaux; Compagnie Générale du Duralumin et du Cuivre; Tréfileries et Laminoirs du Havre; Forges et Chantiers de la Méditerranée.

work-hardened metals are annealed at certain temperatures, and standards are laid down for the annealing temperatures of different metals and alloys. Only then do we start to study the mechanism of recrystallization, mostly from structural evidence, and we now have four or five theories to explain the processes.

I have never found in any paper, however, any measurement in calories of the thermal energy which was necessary to effect recrystallization. I have the feeling that that might provide an answer to a number of questions which are worrying us.

DR. F. C. FRARY * (Member): I think that our people would agree with Dr. Cook in feeling that there are fundamentally the two phenomena, and that grain growth is the only thing that occurs, after the annealing process has resulted in the first recrystallization.

Being educated as a chemist rather than as a metallurgist or metallographer, I am naturally suspicious of techniques in other branches, as well as in my own, and I wonder whether some metallographers might not "kid themselves" in some of their interpretations of photomicrographs. Something of that sort might explain some of the difficulties which we get into in connection with this so-called secondary recrystallization. The metallographer sees a grain as shown in

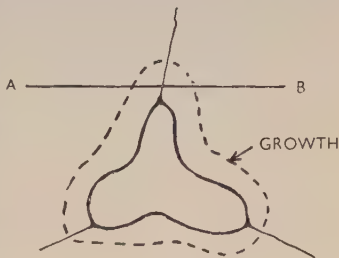


FIG. A.

Fig. A, and he heats it and it grows. That is just grain growth, which you might expect. Suppose, however, he had taken the section at right angles to that, along A-B, showing several grains; he would see the grains grow a little, and then all of a sudden he would see a new grain appearing, and he might say that was secondary recrystallization, when it was only the continuation of this growth which was down below the surface where he could not see it. I am afraid that sometimes there may be errors of that kind in our attempts to decide what is going on in three dimensions by looking at a section on a single plane.

DR. CYRIL STANLEY SMITH,† B.Sc. (Honorary Corresponding Member to the Council for the U.S.A): I would like to demonstrate a model which illustrates in a vivid manner the migration of grain boundaries during grain growth, and the importance of the simple geometric and topological principles imposed by the requirements of surface-energy balance at points where the boundaries meet. The model consists of a froth of soap solution sealed in a tube with a very low residual pressure of air. Surface-tension forces require that three films meeting along an edge common to three cells intersect at an angle of 120° , and that the six edges meeting at the vertices common to four cells make angles of $109^\circ 28'$ with each other. Any other angles are unstable and the films will rapidly move to attain these equilibrium configurations if possible. In the bubble mass, equilibrium is achieved only because there are pressure differences between adjacent bubbles, which maintain curvatures. In the absence of pressure differences it is impossible to reconcile the angles with the varied points of contact between cells except in the single case of the ideal stacking of minimum-area tetrakaidecahedra, following Lord Kelvin.

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† Director, Institute for the Study of Metals, University of Chicago.

Once a temporary balance has been achieved on the basis of these pressure differences, it will slowly change as a result of the diffusion of air from the smaller (high pressure) bubbles to the adjacent larger ones, resulting in a gradual migration of the boundaries until adjacent surfaces come together in an unstable configuration and suddenly re-adjust to a new one. Any departure from the ideal configuration introduces complete instability, and movement will keep up until there are single films normal to the surface across the tube, just as in a fine wire grain growth will continue until grain boundaries are found only in the transverse position. The rate of growth depends on residual air pressure, which is best adjusted until the bubbles grow from a bubble size of about 0.5 mm. to 2 cm. in approximately 10 min. It is important to notice that the migration of boundaries is a highly discontinuous phenomenon. Boundaries will move slowly for long periods until they reach an unstable configuration at their edges and will then very rapidly move to a new stable balance. This can be seen in zinc observed under polarized light, as well as in the bubble model. Although the mechanism of boundary migration in this model is quite different from that involved in grain growth in metals (where there is no equivalent to the pressure difference), the topology is identical and the model has proved very useful in elucidating the geometry of grain growth.

Regarding "secondary recrystallization", it seems probable that this occurs only after primary grain growth has ceased, either because of blocking by inclusions, or because of the effect of specimen thickness, or because of a high degree of preferred orientation. It seems probable that growth stops in all three cases because the boundaries have reached a geometrical configuration which blocks further motion of the normally unstable points of contact whose re-adjustment allows growth. There will be a mass of grains of roughly the same size separated by boundaries whose curvatures are insufficient to overcome the obstacles. If, now, for some reason, one grain should become much larger than its neighbours, the situation becomes once more unstable, for the local balancing forces are removed and the effective curvature of the boundary between the large grain and its neighbours is sufficient to pass over the former block. What allows one grain to become so much larger than its neighbours is obscure, but one possible mechanism would lie in the puncturing of a grain boundary in the event that the grains on each side of it happened, over even an extremely small area, to achieve the same orientation. If the material already has a high degree of preferred orientation, the shift could be a very small one. The sub-grain blocks or the polygonization patches referred to by Dr. Orowan may be involved here. If these can change orientation—and Professor Guinier's Laue photographs obtained by long exposures of aluminium at elevated temperatures indicate some such change of orientation—contiguous areas may occasionally merge. Once this has happened it is equivalent to piercing a soap film and the effect will spread over large distances from the initial upset—again assuming that there exists a mechanism whereby orientation can be changed under the influence of surface forces. This, however, is a completely unsupported hypothesis and the question must await further experimental evidence.

DR. C. J. SMITHELLS,* M.C., F.I.M. (Member of Council): I should like to remind you that every tungsten filament in the lamps in this room takes advantage of this exaggerated grain growth. The filaments, which are about 1 metre long, consist of about 20 crystals, and these single-crystal structures are produced by exactly this type of exaggerated grain growth; they are produced controllably by the million. That control is obtained entirely, from the

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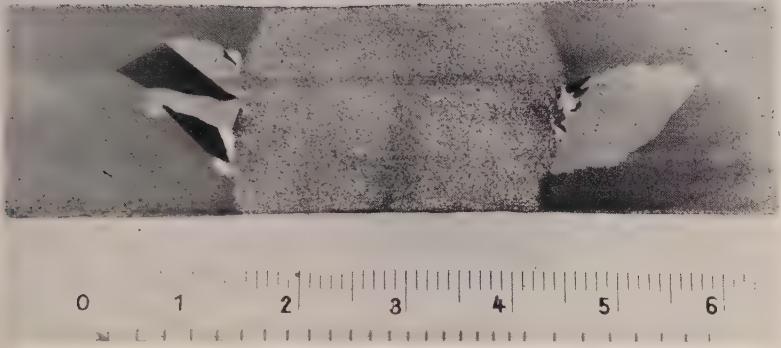


FIG. C.—Face-Centred Nickel-Iron Alloy (47 wt.-% Nickel). First rolling reduction, 90%. Local heating in centre to about 900° C. The second deformation (69%) was high enough to cause a poor cubic orientation in the soft region, when recrystallized at 1200° C.

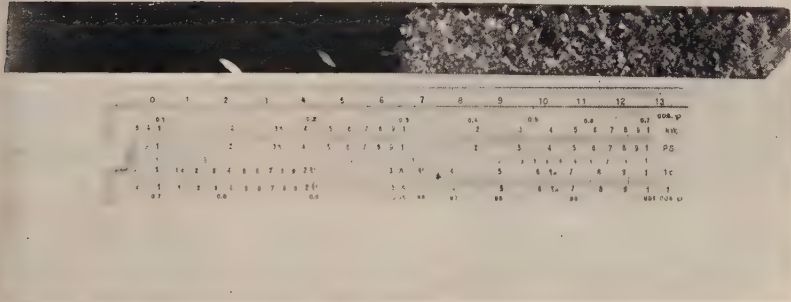


FIG. D.—Nickel-Iron Alloy Sheet, the Left Part Only of Which Was Heated Slowly (in 6 hr.) to 900° C. After cooling, the whole sheet was inserted in a furnace at 1200° C. The initially slowly heated parts mainly show cubic orientation (dark); the remainder of the sheet consists entirely of secondary crystals.



FIG. E.—High-Purity Aluminium, 80% Reduction, 20 sec. at 400° C. (salt bath), completely recrystallized. Mo radiation, 40 kV., 3 cm. distance.

practical point of view, by the control of impurities—I am not prepared to say whether it is their presence or absence.

There are two practical methods of achieving this more or less single-crystal structure in the tungsten filament. Ordinary pure tungsten, prepared as chemically pure as possible, grows large grains, which, as shown in Fig. B representing a longitudinal section of a wire, have more or less straight boundaries. The addition of thorium oxide produces an extremely fine grain, with thousands of grains in the cross-section.

If a small amount of sodium salt is added to the original tungsten before reduction, a type of wire is obtained which, after an appropriate strain, will develop this exaggerated growth, and it happens in less than 0.2 sec. These enormously long crystals are hundreds of times the wire diameter. It can also be done by taking pure tungsten and adding oxides of silica, alkalis, &c., but one cannot detect any of them spectroscopically in the final wire. This produces pure tungsten without any restraining influence, and is the method usually employed.

I support Dr. Chaston in saying that this exaggerated growth is very dependent on the presence or absence of impurities in the material, and I agree with Dr. Cook and Dr. Chaston that this process is simply grain growth. Why certain grains start growing in this abnormal way I do not know, but this growth can be observed as a continuous process following the original recrystallization. Immediately after recrystallization with drawn tungsten wire one gets completely random orientation; there is no preferred orientation in the recrystallized structure, so that there must be some factor, which I think is largely an effect of restraint due to the presence of impurities, which allows certain grains to develop this exaggerated growth.



FIG. B.

DR. L. B. PFEIL,* O.B.E., D.Sc., A.R.S.M. (Member of Council): Is there any fundamental difference between abnormal grain growth produced by annealing following strain and the secondary recrystallization dealt with in this paper by Bowles and Boas? In my experience with abnormal grain growth in iron, the production of abnormally large crystals is easy only in certain types of iron.

I am under the impression that an extremely coarse crystal size is easier to produce when the metal contains something in the nature of finely divided inclusions which will restrain grain growth until a relatively high temperature is reached. Dr. Cook has just spoken of the behaviour of tough-pitch copper which, of course, contains finely divided particles of copper oxide. Another speaker has referred to the behaviour of silver containing silver oxide.

A particularly interesting case is presented by high-purity platinum fabricated by sintering the metal powder followed by hot- and cold-working operations. Provided that the hot-working operations are not carried out at too high a temperature, the cold-worked metal behaves abnormally with respect to recrystallization and grain growth. Severely cold-drawn pure platinum produced in the regular manner from cast ingots will recrystallize in a few minutes on heating at about 600° C., but equally cold-worked platinum produced from a sintered compact does not recrystallize until a temperature in the region of 1000° C. is reached. Furthermore, on recrystallization the sintered and cold-worked platinum gives large crystals, columnar with respect to the direction of cold working. An explanation that has been offered for this phenomenon is that recrystallization and grain growth are restrained by the existence of pores in the metal.

* Manager, Development and Research Department, The Mond Nickel Co., Ltd., London.

I think that in the case of secondary recrystallization a restraining influence, which may be in the nature of inclusions or voids, temporarily prevents the recrystallization and grain-growth tendencies, but when a high enough temperature is reached, or a sufficient length of time has elapsed, the forces overcome the resistance and secondary recrystallization proceeds rapidly.

MR. F. E. MALHERBE * : I should like to comment on what Dr. Smith has just said. I have been doing work on the secondary recrystallization of aluminium, and have noticed that at the stage where large grains are growing one often finds grain boundaries which show a peculiar behaviour, in that they seem to break through. I have a number of examples of grain boundaries which have large gaps in them. It looks as though when there are two grains lying next to one another with similar orientation, the grain boundary actually can break down, producing a large grain with the same orientation.

MR. J. W. JONES,† M.Sc. (Member) : Is there a case for this new terminology? In order to justify the term "secondary recrystallization", distinction must be made between two stages of recrystallization, i.e. primary and secondary.

The term grain growth, more general, and until recently applied to secondary-recrystallization phenomena, is a common mechanism of both primary and secondary stages.

I suggest that the term primary recrystallization refers to the stage in which new crystals begin to form from nuclei, often at grain boundaries or at slip planes. These crystals grow by boundary migration, as shown originally by Carpenter and Elam, into an environment of deformed crystal grain. The stage must be reached, though it may not be reached simultaneously throughout the mass, when the growing grain is no longer surrounded by this environment of deformed material, but these new grains are immediately in grain-boundary contact with other new grains. At this stage primary recrystallization is complete.

The subsequent growth of some of these grains, at the expense of their neighbours, may be recognized as a secondary stage, i.e. not the growth of a grain into an environment of deformed matrix, but growth into a recrystallized matrix. While the mechanism of grain-boundary migration is still the same, the reason why one grain should grow at the expense of its neighbour is still uncertain.

The introduction of such new terms as primary and secondary recrystallization may, however, direct our attention to how much we do and do not know about these respective stages in the changes which take place in cold-worked metal on reheating.

DR. E. OROWAN : I think I ought to give the reasons why I have used the term "secondary recrystallization". The first is that it is the oldest term used; it was suggested 28 years ago by the discoverer of the phenomenon, G. Masing.

Secondly, I think that the use of a term such as "abnormal grain growth" or "exaggerated grain growth" implies an hypothesis. We know that grain growth is a consequence of the tendency of the total interface energy to decrease. Secondary recrystallization is an entirely different type of process. While grain growth leads to boundaries of the foam-structure type, as has been demonstrated by Harker and Parker and by Dr. Cyril S. Smith, secondary recrystallization results in a typical ragged or acicular appearance of the grains. There is no obvious reason to assume that it is related to grain growth.

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† Senior Lecturer in Materials and Metallurgy, College of Aeronautics, Cranfield.

Dr. Chaston has emphasized the influence of the impurities. How very great this is we can see from the observation of the present authors that they could not make pure aluminium undergo secondary recrystallization, while commercial aluminium recrystallized easily. Impurities, however, are hardly the cause of secondary recrystallization; they are only a factor of strong influence upon the process.

I am ready to agree with Dr. Cook that secondary recrystallization may embrace two different phenomena, one producing acicular crystals and the other more equi-axial ones. If he wishes, I am ready to admit that, besides secondary recrystallization, there may be a tertiary recrystallization.

Dr. Chaston asks why the yield stress should be about 15% higher in straight rolling than in uni-axial compression. The v. Mises yield criterion demands that the yield pressure be 15% higher in "plane compression" (where spread is prevented in one direction) than in free (uni-axial) compression. It does not matter from the point of view of the deformed material whether the spread in one direction is prevented by guide plates or by the friction of the rolls, as in straight rolling.

DR. W. BOAS (*in reply*): I wish to thank very much indeed those who have spoken, for their interest in the paper and for their comments. This type of discussion and criticism, for which this Institute is famous, is rare in Australia, but we hope to foster it in our new Australian Institute of Metals.

With regard to one of the main points of the discussion, terminology, I do not agree with the speaker who said that the phenomenon follows continuously on grain growth or on primary recrystallization. I believe that what we call secondary recrystallization is fundamentally a different process and therefore needs a new name. Some of the grains grow; it is not, as in grain growth, a continuous growth of all the crystals and a coarsening of the structure; what happens is that some of the crystals, for some reason or other, just grow. If you call it "exaggerated grain growth" that is another name, but I agree with Dr. Orowan that this term already implies an explanation which is not warranted at the present time.

When we started writing the paper we found great difficulty in dealing with previous literature, but as there is the famous and comprehensive book which Professor Burgers has written on the subject, we did not discuss the theories or explanations based on impurities with which Burgers has dealt extensively.

The thickness of the strips, about which Dr. Chaston asked, was quite small. On p. 504 it is stated that the original thickness of the specimens was $\frac{1}{2}$ in., and the reduction in thickness by rolling is given in the same paragraph. We ended up with specimens about 0.02 in. thick.

I am not clear about the situation with respect to tungsten, which Dr. Smithells mentioned. I think that in the case of tungsten you start with powder, which means that you make your tungsten by powder-metallurgy methods. Dr. Pfeil mentioned in the case of platinum that the properties of such material may be completely different from those of material made from a casting.

I agree with the remarks of Dr. Lepp with regard to energy measurements; I think that that is quite fundamental. We are carrying out this type of experiment in Melbourne. It is necessary to investigate the primary recrystallization before secondary recrystallization in terms of energy stored in the material. We know that the usual recrystallization diagram is not of much use, e.g. because it does not show the grain-size effect. One has to measure very carefully the energy stored before primary recrystallization—measurements of the type which G. I. Taylor has made in order to find out what is actually going on. We hope to push the accuracy a little higher, but it is a very awkward thing to do.

I cannot reply from memory to Dr. Chaston's question about the purity of the material. The straight and compression rolling were carried out on the same material treated in the same way, and therefore the results obtained on the one material are strictly comparable, whatever the impurities are; but that might explain the difference between the behaviour of our material and the material of which Dr. Cook spoke.

In reply to Dr. Cook, the large crystals shown in one of his sheets were produced by annealing after cross rolling. I want to emphasize again the difference between cross rolling and what we call "compression rolling". Some objection may be taken to the term "compression rolling", which was introduced by Barrett.

There are two comments which I should like to make on Dr. Orowan's earlier remarks. He said that the yield stress is different in compression rolling, and, of course, that is so. The hardness measurements which we have made, and which are given in the Appendix to our paper, show that the compression-rolled material always work-hardened less than the straight-rolled material, in accordance with the theory to which Dr. Orowan referred; nevertheless, the grain-size after primary recrystallization was amazingly similar in the straight- and the compression-rolled specimens.

What Dr. Orowan said with regard to the inhomogeneity of rolling is, of course, also true. I do not think that it affects our experiments, because of the thinness of our strips. I might mention in that respect that Collins and Honeycombe have made experiments on those lines which have been reported in a paper which has just been published.* Actually, this inhomogeneity was found to be less important in rolling than in drawing, where the effect is very marked.

CORRESPONDENCE.

PROFESSOR W. G. BURGERS,† Dr. Chem. (Member): This paper makes a valuable contribution to our knowledge of the phenomenon of "secondary recrystallization" in various metals. As pointed out by the authors, their results are to a considerable extent in agreement with, and moreover give new support to, some conclusions regarding this phenomenon deduced from earlier work by myself and co-workers with aluminium, in particular as to the observation that a pronounced tendency for the formation of large secondary crystals goes hand in hand with the presence of a pronounced preferential orientation of the primary crystallites. In an effort to understand this, the authors, on p. 515 of their paper, differentiate between "the tendency for the secondary crystals to grow" and "the ease with which they grow". Although the precise meaning of this discrimination is not quite clear to me from what they say about it, I wholly agree with their conclusion (same page) that the rate of migration of a boundary in any direction is influenced by the change in orientation which occurs on crossing the boundary in that direction. According to that view, secondary crystals have, with regard to that of the primary texture, a favourable orientation for growth.

The authors also accept the view that those parts of the original texture which have been left unconsumed during the growth of the primary crystals, act as "nuclei" of the secondary crystals, and thus, as they express it so clearly, have been "left over" from the deformation texture (their interpreta-

* J. Collins and R. W. K. Honeycombe, *J. Council Sci. Indust. Research Australia*, 1948, **21**, 59.

† Professor of Physical Chemistry, Laboratorium voor Physische Chemie, Technische Hoogeschool te Delft, Holland.

tion of this fact is apparently somewhat different from that put forward by myself). With regard to this question, I may point out that in recent work carried out by myself, together with May and Tiedema,* it has been found that in aluminium the unconsumed parts of the original deformation texture occupy *approximately* (within about 5°) a spinel-twin orientation with regard to that of the growing primary crystals; in other words, a growing crystal is apparently incapable of consuming a lattice part in *approximate* twin orientation. On the other hand, it has been found † that on "collision" of a growing crystal with a lattice block in *perfect* twin orientation, this latter may be induced to crystal growth itself ("stimulated", as we called it), and we believe that these two phenomena, or a suitable "combination" of them, may play an important part in the recrystallization process. This is perhaps to some extent confirmed by the observation, made by the authors (Fig. 20 of their paper), that in the case of copper the orientation of definite secondary crystals could be obtained from that of the primary "cubic texture" by a rotation round a line perpendicular to a 111 plane over an angle different from that (60°) required to establish an exact twin relationship. It ought to be mentioned here that an analogous relationship was observed earlier by Custers and Rathenau ‡ in their study of the texture and crystal growth of nickel-iron foil (with this alloy other orientations are obtained as well, as mentioned by the authors on p. 502 of their paper).

Finally, I should like to call the attention of the authors to a comprehensive paper on recrystallization by C. Petersen,§ which was only recently made available to me. In this paper the recrystallization phenomena are discussed from the general point of view of stress release, and in various places the paper may have a bearing on the present subject (e.g. with regard to the inability of a growing crystal to consume definite lattice parts).

DR. G. W. RATHENAU ||: The authors are to be congratulated on their most interesting paper. Their suggestion that the coincidence of the important crystallographic planes of the secondary crystals and the primary recrystallized matrix is a condition for fast secondary grain growth is substantiated by earlier investigations.¶ It has been shown that spinel twins may grow in a cubic texture as secondary crystals and that, as in the case studied by the authors, the growth of the secondary crystals is probably most rapid along the common octahedral plane.

Dr. J. F. H. Custers and the writer completed some work on the secondary recrystallization of face-centred nickel-iron alloys in 1941. This research, which could not be published during the occupation of the Netherlands, will be published in *Philips Research Reports*. The above mentioned condition for secondary grain growth was also found by us. Among the types of secondary crystals there was one which had the (112) plane parallel to the (100) plane of the primary cubic texture, whereas other important crystallographic planes, such as the (111) plane of the secondary crystal coincided with important planes of the primary texture.

That it is this relationship between the orientation of the secondary crystal and the orientation of the primary matrix which determines certain types of secondary crystals became clear when it was found that the same kinds of orientation for secondary crystals were always encountered, whether they originated in the normal way, or under quite different conditions of stress. They grew from predictable places, where the primary recrystallized material was de-

* *Nature*, 1948, 162, 740.

† *Nature*, 1946, 157, 76.

‡ *Physica*, 1941, 8, 759, 771.

§ *Metallforschung*, 1947, 2, 289.

|| Philips Research Laboratories, N. V. Philips' Gloeilampenfabrieken, Eindhoven, Holland.

¶ G. Wassermann, *Z. Metallkunde*, 1936, 28, 262.

formed by a prick with a pin and subsequently annealed; they grew from spots where the amount of cold rolling was locally reduced by annealing those spots in between the stages of the rolling process; and they grew from places which, by cutting in the rolled state with scissors, had undergone a strain release before recrystallization.

It being clear that the presence of germs with "correct orientations" within the primary recrystallized material is a necessary condition for pronounced secondary grain growth, experiments were designed to find out what additional conditions have to be fulfilled. The secondary crystals have indeed lower free energy than the primary ones. This conclusion was drawn from experiments in which pinholes were pricked in primary recrystallized material as well as in secondary crystals grown within it. Small recrystallized crystals formed on subsequent annealing around the pinholes. They were often able to grow to large dimensions in the region of the cubic orientation, but not in the region of the secondary crystals.

The imperfections of the primary recrystallized material probably have several causes. Firstly, the large number of the recrystallizing nuclei prevents each of them from growing far enough to become perfect at its surface.* Secondly, to become complete the primary recrystallization needs a broad interval of time and/or temperature.† The elastic strains released from the unrecrystallized grains deform plastically the material already recrystallized.‡ Thirdly, the total surface energy of the primary recrystallized material may be appreciable.

An explanation may now be attempted of why crystals growing from pinholes in primary material are more perfect than the primary grain itself; the pin deformation decreases so rapidly with the distance from the hole, that a crystal growing outwards can do so without encountering competing nuclei or releasing appreciable elastic energy. This view suggests that the least deformed parts of the normal material, where the smallest density of recrystallization nuclei and the longest times of recrystallization are encountered, produce recrystallization nuclei which are not hampered in their growth by competing nuclei and therefore become the most perfect parts of the primary recrystallization structure. These parts probably act as nuclei for secondary recrystallization.

The experiment referred to above, where in between the stages of the rolling process part of the material was annealed, is a model experiment to test this view. Fig. C, Plate LII, shows secondary crystals growing from the least-deformed, locally annealed part in the centre into the region of cubic orientation.

Other types of experiment, resembling those on "forced secondary recrystallization",§ show that if rolled material is made "strain-free" locally by a deviating type of deformation, such as cutting with scissors (interference of strains),|| secondary crystals grow from those places on annealing.

Other experiments, resembling those of Carpenter and Elam,¶ concerned the influence of heating velocity. If the least-deformed parts of the deformed material, from which secondary crystals may develop, have really a higher activation energy Q_n for nucleation (number of growing nuclei/c.c./sec.) than the remaining nuclei, and if moreover the activation energy for grain growth (increase in radius of recrystallized grain, cm./sec.) is fairly independent

* W. G. Burgers, "Handbuch der Metallphysik". Band 3, Teil 2, p. 246. Leipzig: 1941.

† J. F. H. Custers and G. W. Rathenau, *Physica*, 1941, **8**, 759.

‡ M. Póányi and G. Sachs, *Z. Metallkunde*, 1925, **17**, 227.

§ H. G. Müller, *Metallwirtschaft*, 1940, **19**, (25), 509.

|| J. A. M. van Liempt, *Z. anorg. Chem.*, 1931, **195**, 366.

¶ H. C. H. Carpenter and C. F. Elam, *J. Inst. Metals*, 1920, **24**, 83.

of deformation and smaller than Q_{ns} , as must be expected,* the number of secondary crystals will rise enormously with temperature. In other words, if the temperature is such that the first-recrystallizing parts of the material are able by grain growth following recrystallization to devour the less-deformed, unrecrystallized portions, practically no secondary crystals will be formed. As an example of the influence of temperature, which was found to be very marked, Fig. D, Plate LII, shows a sheet, one part of which was slowly heated to above the primary recrystallization temperature. Then the whole sheet was inserted in a furnace at 1200° C. It can be seen that the part which had undergone primary recrystallization at high temperature shows an enormous number of secondary crystals, mostly of the well known orientations. The pre-heated part on the left is fairly free from secondary crystals.

On variation of the degree of rolling, large secondary crystals are found only in certain instances. At low rolling reductions so many secondary crystals occur that they give the impression of normal grain growth. They have, however, a special orientation. Sheets with a high rolling reduction, on the other hand, before annealing probably do not contain areas with a low enough internal energy and the correct orientation. Cutting of such rolled sheet with scissors, however, may produce secondary crystals on annealing.

Coming back to the paper under discussion, the somewhat irregular process of compression rolling recalls experiments in which two different kinds of deformation were applied. By "interference of strains" the deformation stresses were locally reduced (and at other places raised), so enhancing the nucleation of secondary crystals. Do the authors think it impossible in this connection that the rather large inclusions within the secondary crystals of compression-rolled commercial aluminium (as in Fig. 18 of their paper) represent some type of secondary crystal themselves? Is the crystallographic relation between them, their secondary "mother crystal", and the matrix known?

PROFESSOR PAUL A. BECK †: In a recent investigation at this laboratory ‡ on coarsening in aluminium-manganese alloys, the conclusion was reached independently but in agreement with Bowles and Boas, that the decrease in the incubation period of coarsening, resulting from increasing reduction by rolling, is probably connected with the increasingly well developed preferred orientation of the recrystallized material. This work, therefore, gives further support to the main conclusion reached by Bowles and Boas.

It seems, however, that in aluminium containing small amounts of a dispersed iron- or manganese-compound, a well developed recrystallization texture is not *necessary* for coarsening. This is corroborated by Bowles and Boas's "compression rolled" specimens of commercial aluminium. What appears to be necessary is the *presence of a dispersed second phase with grain-growth inhibiting properties*. The commercial aluminium used by Bowles and Boas, containing 0.5% iron and 0.2% silicon, undoubtedly contained significant amounts of the aluminium-iron and aluminium-iron-silicon compounds. Since a well developed texture is not a necessary condition of coarsening, its lack in the purer grade of aluminium used by Bowles and Boas should not be blamed for the absence of coarsening in this material. As a matter of fact, it is possible to develop a well marked recrystallization texture in high-purity aluminium after only 80% reduction by rolling, as shown in Fig. E, Plate LIII. Nevertheless, as already found by Feitknecht § and con-

* W. A. Anderson and R. F. Mehl, *Trans. Amer. Inst. Min. Met. Eng.*, 1945, 161, 140.

† Associate Professor of Metallurgy, University of Notre Dame, Indiana, U.S.A.

‡ P. A. Beck, M. L. Holzworth, and P. Sperry, *Metals Technol.*, 1948, 15, (6); *A.I.M.M.E. Tech. Publ. No. 2475*.

§ W. Feitknecht, *J. Inst. Metals*, 1926, 35, 131.

firmed in recent work at this laboratory * as well as by Bowles and Boas, this material does not coarsen even after very prolonged annealing periods. It appears certain that the absence of coarsening, or discontinuous grain growth, in pure aluminium is a direct result of the absence of an inhibiting second phase. Lacking inhibition, pure aluminium shows continuous or gradual grain growth, typical of single-phase alloys.†

The coarsening investigated by Bowles and Boas in copper appears to be of a rather different nature. Although the copper used by them was presumably of a tough-pitch variety (it is stated to be 99.95% pure) containing Cu_2O particles, coarsening of the same type can be obtained also with oxygen-free types of copper.‡ It appears that, in contrast to coarsening in aluminium, the presence of a dispersed second phase is not necessary for the type of coarsening investigated by Bowles and Boas in copper. In further contrast to the coarsening in aluminium, a high degree of deformation by straight rolling, resulting in a well developed cubic recrystallization texture, seems to be a necessary condition here.§ This explains the absence of coarsening in compression-rolled copper, as shown by Bowles and Boas's data. The considerable difference in the mechanism of coarsening in aluminium and in copper in Bowles and Boas's experiments is also suggested by their excellent Figs. 15 and 17. These strikingly demonstrate the very different character of the two types of large grains, formed in both instances in a fine-grained matrix of well developed texture. It should be noted that the type of coarsening which occurs in aluminium was also found in tough-pitch copper by Cook and Macquarie.|| Here the shape of the large grains was similar to that in aluminium. (See Fig. 4 in the paper by Cook and Macquarie.) That the phenomenon was connected with the Cu_2O particles dispersed in the tough-pitch copper used is shown by the fact that this type of coarsening could not be reproduced in oxygen-free copper.

In the case of aluminium-manganese alloys, the processes taking place during the incubation period preceding coarsening have been investigated in considerable detail.¶ The results make it appear extremely unlikely that any residual portions of the deformed structure could survive to serve preferentially as "nuclei" of the large grains in coarsening. In a typical case, recrystallization was complete in a small fraction of the first minute of the coarsening anneal. During the following 25 min. or so, the fine recrystallized grains grew to about double their original size. During this period of initial grain growth every part of the structure must have been swept by at least one advancing grain boundary. If recrystallization would have left over some residues of the deformed material, these must certainly have been eliminated by the initial grain growth. But if any minute trace of the deformed material could still have survived, the following period of incubation (2-55 days), many thousand times longer than the time necessary for recrystallization at the same temperature, should undoubtedly have afforded ample opportunity for recovery. If it is admitted that the preferential growth of crystals of certain orientation is a result of their relative orientation with respect to the matrix, then it is unnecessary to make the improbable assumption that portions of the deformed material survive recrystallization, grain growth, and long periods of recovery at high temperature. After all, even in a well developed texture there is always a considerable scatter of orientations, and

* P. A. Beck, J. C. Kremer, L. J. Demer, and M. L. Holzworth, *Metals Technol.*, 1947, 14, (6); *A.I.M.M.E. Tech. Publ.* No. 2280.

† P. A. Beck, J. C. Kremer, L. J. Demer, and M. L. Holzworth, *loc. cit.*

‡ M. Cook and C. Macquarie, *Trans. Amer. Inst. Min. Met. Eng.*, 1939, 133, 142.

§ O. Dahl and F. Pawlek, *Z. Metallkunde*, 1936, 28, 266.

|| M. Cook and C. Macquarie, *loc. cit.*

¶ P. A. Beck, M. L. Holzworth, and P. Sperry, *loc. cit.*

there will always be present some grains of the particular relative orientation which can grow fastest.

Finally, as one who is engaged in teaching metallurgy, I should like to make an appeal to those who publish papers on grain growth, in favour of a unified and unambiguous nomenclature in this field. The term "secondary recrystallization" was originally introduced by Masing in describing his work on tin.* However, it was later decisively proved by van Arkel and Ploos van Amstel† that the phenomena investigated by Masing were actually ordinary recrystallization phenomena. The grains formed were large because the strain applied by Masing was small. The use of the term "recrystallization" only in instances where the driving energy of the process is strain energy (annealing after plastic deformation) is now well established and should be retained. Where grains grow at the expense of other essentially unstrained grains, the term "grain growth" may be applicable. If it is desired to emphasize the characteristically abrupt emergence of very large grains directly from a fine-grained matrix under certain conditions (such as those described by Bowles and Boas), in contrast to the gradual and continuous nature of the grain-growth process in other instances, the term "discontinuous grain growth" or "coarsening" might be recommended. The latter is very widely used to describe similar phenomena in certain steels. The terms "abnormal grain growth" or "exaggerated grain growth" are also confusing to the student because through the years they have been used indiscriminately to describe the formation of large grains by either coarsening or recrystallization after small deformations.

The AUTHORS (in written reply to the discussion and in reply to correspondence): We should like first of all to thank Professor Burgers, Dr. Rathenau, and Professor Beck for their valuable contributions, and we are particularly glad that Professor Burgers' studies and Dr. Rathenau's unpublished work are in agreement with our results.

In further reply to Dr. Chaston, the purity of the copper was stated (p. 503) to be 99.95%, not 99.5%. Subsequent detailed analyses‡ of the copper and silver showed them to be purer than we had at first thought, namely: *copper*: 99.99%; very faint traces of Ag, Fe, Mg; no Pb, Sn, Sb, As, Bi, Mn, Al, Ni, Cd; *silver*: 99.94%; faint traces of Cu, Pb, Fe, Mg; no Zn, Sn, Sb, As, Bi, Mn, Al, Ni, Cd.

We cannot agree with Professor Beck's suggestion that secondary recrystallization is due to impurities present as a second phase. If it be assumed that particles of this phase inhibit the growth of crystals, it cannot be explained why the same particles do not protect these crystals from absorption, why the orientation of the secondary crystals is closer to that of the deformation structure than to that of the primary structure, and why the rate of secondary recrystallization in our single crystals of commercial aluminium was greater than in the polycrystalline specimens of identical material. The impurity hypothesis can explain how large grain-size results from uniform growth of all the crystals, but not why a few crystals in a fine-grained matrix grow at the expense of the majority.

We agree with Professor Beck that the term "recrystallization" should be used only where strain energy is the driving force. This is just the reason why we think that the process under discussion should be called "secondary recrystallization" and not some type of "grain growth".

In reply to Professor Burgers' question, by "tendency for the secondary

* G. Masing, *Z. Metallkunde*, 1920, 12, 457.

† A. E. van Arkel and J. J. A. Ploos van Amstel, *Z. Physik*, 1928, 51, 534.

‡ We have to thank the staff of the Defence Research Laboratories, Mairymong, Victoria, for carrying out these analyses.

crystals to grow" we mean the driving force behind the growth process; "the ease of growth" determines the rate of growth if the tendency is constant.

Regarding Dr. Rathenau's question, we do not think it impossible that the small crystals included in the large secondary crystals in Fig. 18 of our paper are themselves secondary crystals. Their orientation has not been determined.

We are looking forward to the full publication of the work by Drs. Rathenau and Custers, which seems to us to be the most direct approach to the problem of secondary recrystallization.

CORRESPONDENCE ON PAPER BY MR. P. G. FORRESTER AND MR. L. T. GREENFIELD:
 "FURTHER EXPERIMENTS ON THE AD-
 HESION OF TIN-BASE BEARING ALLOYS."

(*J. Inst. Metals*, this volume, p. 525.)

MR. H. J. ROAST,* F.I.M., F.C.S. : Why is it that in the case of bronze of composition zinc 3, tin 7, lead 15%, copper remainder, it is essential to have 3% zinc if proper adhesion of Babbitt to bronze is to be achieved?

The bronze is cast in sand, machined, rubbed with muriatic acid "killed" with zinc, immersed in a tin bath until it has reached the temperature of the bath, and then placed against a mandrel. The Babbitt is then poured. The temperature of the tin bath (tin 10%, lead remainder) is 600°-650° F. (316°-343° C.), that of the Babbitt (tin 2, antimony 10%, lead remainder) is 700°-800° F. (371°-427° C.), and that of the mandrel 175°-275° F. (80°-135° C.).

By proper adhesion is meant that on fracturing the bronze and separating the Babbitt from it, a rough surface is exposed, grey in colour, showing that the solder has attached itself to the bronze and the Babbitt to the solder.

If the zinc content is allowed to fall to 1.5%, the fracture shows a smooth, copper-coloured surface, with no sign of adhesion of solder to bronze or of bronze to solder. Have the authors any explanation of this?

MR. P. G. FORRESTER (*in reply*) : The point raised by Mr. Roast is very interesting, especially in view of the fact that we observed zinc to have little effect on the adhesion of lead-free bronze. We notice, however, that in the example cited by Mr. Roast, the bronze was not pickled before tinning, the killed spirit being relied upon to remove the oxide film. It may well be that the oxide film of a zinc-containing bronze is more readily removed by the flux than that of a similar zinc-free bronze.

* Consulting Metallurgist, Montreal, Canada.

CORRESPONDENCE ON PAPER BY DR. E. VOCE : "THE RELATIONSHIP BETWEEN STRESS AND STRAIN FOR HOMOGENEOUS DEFORMATION."

(*J. Inst. Metals*, this volume, p. 537.)

DR. MAURICE COOK * (Member of Council) : The first part of this interesting paper is concerned with homogeneous compression, and in that connection Dr. Voce has examined results which Mr. Larke and I obtained for a variety of commercial non-ferrous materials using an experimental procedure which obviated effects of size of test specimen and friction. It is quite clear that no single numerical value can be assigned to compressive strength as it can, for example, to ultimate tensile strength, and the results we obtained were, therefore, presented in the form of basic compression curves. Dr. Voce has studied the relationship between stress and strain involved in homogeneous deformation and has derived a formula for computing homogeneous compression curves. The value of the formula, however, seems to be limited, since its numerical solution necessitates the use of a final constant stress which can only be established by first experimentally determining the complete curve. From the formula, he derives, however, a dimensionless numerical constant k which varies for different materials, and it will be interesting to learn what significance this may prove to have.

Curves in which the resistance to homogeneous deformation is plotted against the magnitude of the deformation are of use in studying metal-manipulating processes, such as rolling, since data of this sort are required for formulæ employed in the calculation of rolling loads and torque. It was, indeed, the lack of such data that led Mr. Larke and myself to devise a method for determining basic curves of resistance to deformation, which, since it is not uncommon in the rolling of metals to effect reductions between anneals of 70% or more, extended to reductions in thickness of this order. The extent of homogeneous deformation in an ordinary tensile test is too small to be of any use in calculations of this kind, and Dr. Voce has suggested that if a curve of homogeneous tension could be developed to cover deformations extending well beyond the limit of uniform elongation, it would be comparable with that obtained by the method of homogeneous compression which Mr. Larke and I developed. Dr. Voce has succeeded in devising a method of extending the course of the true tensile curve indefinitely beyond the point of necking to represent homogeneous deformation in this region. The validity of the method, however, requires experimental confirmation, for only a limited amount of supporting data is available and this Dr. Voce has cited. If it could also be definitely established that such a homogeneous tensile curve does, in fact, correspond to a basic compression curve, it would be a great advantage, since it would enable the homogeneous deformation characteristics of metals to be obtained by use of a single tensile test in place of the more complex compression test which involves three or four cylindrical specimens of different sizes. In most operations involving the working of metals the deformation is not homogeneous in character, but in order that complex deformation may be clearly comprehended and analysed, it is

* Director, Imperial Chemical Industries, Ltd., Metals Division, Birmingham.

necessary first to have a knowledge and understanding of simple or homogeneous deformation. As a contribution to this subject Dr. Voce's paper is most valuable and welcome, and I heartily support his expression of hope that it will stimulate further experimental work.

MR. J. H. PALM,* Ir. (Member): The empirical derivation of formula (9) by Dr. Voce is very interesting, since this formula has real fundamental significance. The formula was derived some time ago in our laboratory as well, but along purely theoretical lines. Following up suggestions of the present writer and starting from a simplified picture of the metal and the

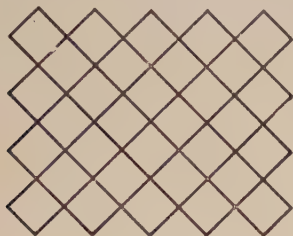


FIG. A.

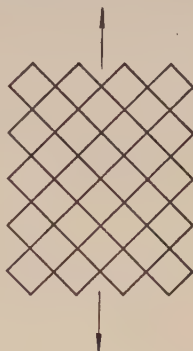


FIG. B.

strain-strengthening mechanism, Dr. A. van Wijngaarden, former research engineer of our Institute, arrived at the formula as follows. The metal is assumed to be homogeneous and initially isotropic. Gliding then occurs in the planes of maximum shear stress, which makes an angle of 45° with the principal stress directions. It is further assumed that the number of glide planes per unit length a remains constant, as is schematically illustrated in Figs. A and B for the two-dimensional case. Strain strengthening is thought to be caused by knots, which come into existence and remain intact in the glide planes during gliding and resist gliding, each with a constant force.

When τ_0 is the original shear stress, n the number of knots per unit of surface in the glide planes, and t_k the resisting shear force per knot, we have for the actual shear stress τ :

$$\tau = \tau_0 + nt_k \quad . \quad . \quad . \quad (1)$$

Let N be the total number of atoms per unit of surface in the glide planes, m the number of atoms per knot, and λ the distance over which gliding in each plane has proceeded. It is now plausible that the formation rate of

knots $\frac{dn}{d\lambda}$ is directly proportional to $(N - m \cdot n)$, thus:

$$\frac{dn}{d\lambda} = k_1(N - m \cdot n) \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which k_1 is a constant.

* Head, Material Department, National Luchtvaartlaboratorium, Amsterdam, Holland.

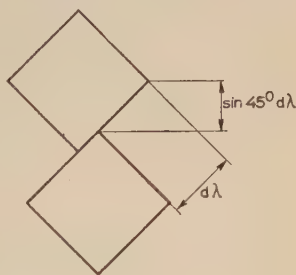


FIG. C.

For an increase of shear $d\lambda$ in each glide plane (Fig. C), the increase of length per unit length, or the increase of natural strain $d\eta$, becomes :

$$d\eta = a \sin 45^\circ d\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From (2) and (3) it follows that :

$$\frac{dn}{d\eta} = a \sin 45^\circ k_1(N - m \cdot n) = k_2(N - m \cdot n).$$

Integrated :

$$\log_e(N - m \cdot n) = -m(k_2\eta + k_3)$$

or :

$$n = \frac{N - e^{-m(k_2\eta + k_3)}}{m}.$$

k_2 is a constant, k_3 is the integration constant. When $\eta = 0$, $n = 0$ also, thus :

$$N = e^{-mk_3}$$

or :

$$n = \frac{N}{m}(1 - e^{-mk_2\eta}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

From (1) and (4) it follows, writing $\frac{1}{\eta_c}$ for mk_2 , that :

$$\tau = \tau_e + \frac{Nt_k}{m}(1 - e^{-\eta/\eta_c}) \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

For $\eta = \infty$, $\tau = \tau_e$, thus :

$$\tau_i = \tau_e + \frac{Nt_k}{m} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

From (5) and (6) it follows that :

$$\tau = \tau_i - (\tau_i - \tau_e)e^{-\eta/\eta_c} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

For uni-axial stressing (7) becomes :

$$S = S_i - (S_i - S_e)e^{-\eta/\eta_c} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which is, apart from a different notation and form, equivalent to Dr. Voce's formula. We called S_i the *asymptotic stress* and η_c the *characteristic strain*.

The formulæ (7) and (8) enable one to derive the stress-strain relations for bi-axial and tri-axial loading conditions when two principal stresses are equal and as long as the strain remains uniform, and to determine the mean tri-axiality during necking.

Formula (8) was applied to a great number of tensile tests, and, in agreement with Dr. Voce, the accordance was found perfect beyond a low permanent strain.*

PROFESSOR FRANCESCO MAZZOLENI † (Member) : In his interesting paper, Dr. Voce suggests an equation of the following type for the analytical expression of the relationship between stresses and strains :

$$\ln \frac{S_\infty - S_0}{S_\infty - S} = k \ln \frac{L_0}{L}$$

where S_0 is the stress corresponding to the commencement of plastic deformation, S the instantaneous value of the stress at a given instant, S_∞ the limiting value of the stress for infinitely large deformation, L_0 the initial length, L the instantaneous value of the length, and k a dimensionless numerical constant.

* *Appl. Sci. Research*, 1948, [A], 1, (3), 198.

† Special Technology Department, Faculty of Engineering, University of Naples, Italy.

I desire to mention that in a paper published in 1941 entitled "Le deformazioni plastiche a freddo e l'incrudimento dei metalli",* of which a reprint has been sent to Dr. Voce, I deduced an expression, not very different analytically from the one which he now proposes, which resulted from the development of two hypotheses rather than from a search for a formula which would explain the form of experimental stress-strain diagrams. The two hypotheses were:

(1) The element of work done in an infinitesimally small plastic deformation is transformed in part into the potential energy of work-hardening and in part into heat, both fractions varying linearly with the total amount of energy of work-hardening stored up in the metal from the commencement of deformation.

(2) The resistance to deformation (true stress) increases linearly with the content of energy of work-hardening.

From these hypotheses it can be deduced that, retaining the symbols used by Voce, the following formula holds:

$$\ln \frac{S_{\infty} - S_0}{S_{\infty} - S} = \int_{L_0}^L \frac{AS}{E_1} dL$$

in which E_1 denotes the maximum energy of work-hardening which it is possible to accumulate in the mass of metal subjected to deformation.

Since $A = \frac{A_0 L_0}{L}$, and using E_1' to indicate the value of E_1 referred to unit volume of metal, it follows that $E_1' = \frac{E_1}{A_0 L_0}$, and the formula can be written:

$$\ln \frac{S_{\infty} - S_0}{S_{\infty} - S} = \frac{1}{E_1'} \int_{L_0}^L S \frac{dL}{L}.$$

This formula differs from that proposed by Voce in that S occurs in the integrand, and that the constant by which the integral is multiplied is not a pure number.

If the integration is performed in the manner indicated in my paper, an expression for the relationship between the true stress S and the strain ratio $\frac{L}{L_0}$ is obtained:

$$S = S_{\infty} \frac{a \left(\frac{L}{L_0} \right)^b}{1 + a \left(\frac{L}{L_0} \right)^b}$$

in which a and b are constants.

From tests carried out on a number of metals it has been shown that this formula represents experimental stress-strain diagrams reasonably satisfactorily. Further, values obtained for the energy stored in the metal as the result of cold working, which can be calculated from the values of the constant b as determined from the experimental diagrams, agree satisfactorily with those obtained by direct measurement, which I regard as a good confirmation of the validity of the two hypotheses on which the treatment is based.

MR. N. H. POLAKOWSKI,† Dipl. Ing.: The stress-strain equation proposed by Dr. Voce is of interest not only because of its agreement with the curves of

* F. Mazzoleni, *Met. Ital.*, 1941, 33, 390.

† Research Student, University College, Swansea.

homogeneous deformation derived by Cook and Larke, but also from a more fundamental viewpoint. It is "an attempt to adapt the "law of organic increase" to plastic deformation, and the curve obtained is identical with a heating curve or with a time-current curve in an inductive circuit. Appropriate formulæ will be obtained by replacing e by time and S by T or I respectively, and S_∞ will represent the equilibrium stress.

This general character of the formula in question is undoubtedly its advantage when compared with many others of purely numerical character but without physical significance.

In support of Voce's figures it may be added that the curves plotted by Cook and Larke have to be considered with some precautions in the range

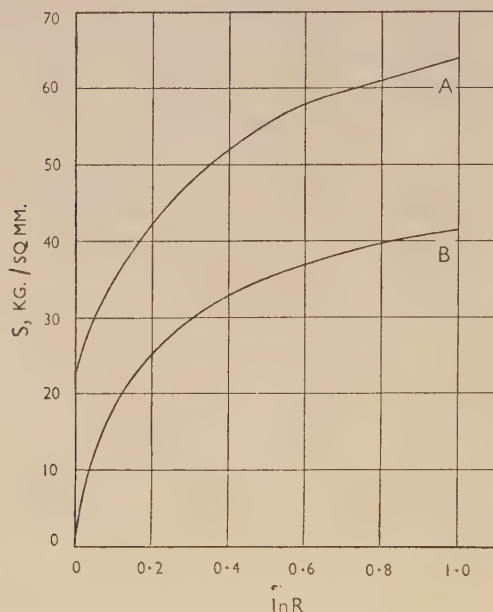


FIG. D.—Curves of Siebel and Pomp. Compression.

A. Krupp's soft iron.

B. Copper.

of heavy deformations. When extrapolated by means of the Lagrange formula, the curve (h) of 60% deformation in Fig. 14 of their paper gave $S = 20.65$ tons/in.² for $d_0/h_0 = 0$, a figure which is appreciably nearer to $S_\infty = 19.7$ quoted by Voce. The following points were used for the extrapolation: $0.5/25.7 - 1.0/30.3 - 2.0/41.3 - 3.0/60$. However, little success was attained when Voce's method was applied to the curves published by Siebel and Pomp* (see Fig. D). Two curves were checked, those of copper and Krupp's soft iron (KW), using both free-hand extrapolation and numerical methods to establish suitable values for S_∞ and k , and in both cases considerable discrepancies were found, of the order of 10–15%.

It may be added that Siebel's curves were drawn direct from experi-

* Mitt. K.-W. Inst. Eisenforsch., 1927, 9, 169.

mental results obtained without any extrapolation, which always involves some uncertainty.

Still different curves were obtained by Körber and Müller * (see Fig. E), which obviously do not fit the proposed equation. The method used was an indirect one, but the principle of geometrical similarity was also applied.

Nevertheless, Voce's formulæ are very attractive for their simplicity, and further investigation may supply more experimental evidence.

The examples taken from the work of MacGregor and his collaborators are very striking indeed, but it should be remarked that their curves were

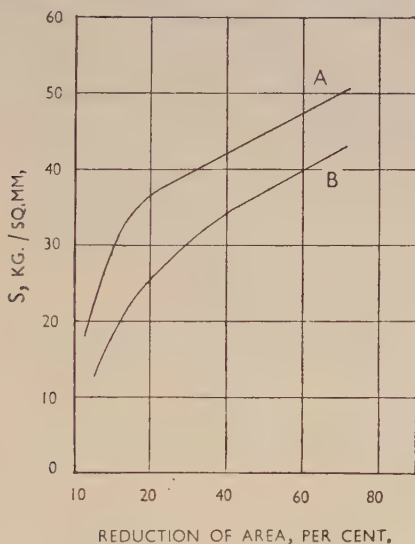


FIG. E.—Curves of Körber and Müller. Tension.

A. Krupp's soft iron.

B. Copper.

experimental only, and MacGregor neither claims homogeneity nor recommends any particular equation for the *whole* curve (rather two different curves). The relation $\left(\frac{dS}{de}\right)_m = S_m$ cannot be compared with MacGregor's

$\frac{\partial S}{\partial e} = m$, which is only a complicated expression for the slope of a straight line.

The link between the curved and rectilinear portions is by no means clear, as rightly pointed out by Dr. Voce, but it would not be safe to assume that the deformation remains homogeneous far beyond the maximum load, as suggested on p. 551.

There is, however, an apparent contradiction (though possibly of little practical significance) between MacGregor's experimental curves and his "two load method". This should fit $\left(\frac{dS}{de}\right)_m = S_m$ if it were true that the straight line is a tangent to the curved line at the point of inflection, which, it is

* *Mitt. K.-W. Inst. Eisenforsch.*, 1926, 8, 187.

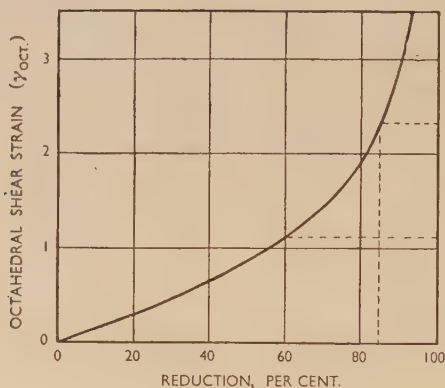
claimed, is identical with S_m . This would appear to be the point to which Dr. Voce's criticism should be related.

The symbol e used for logarithmic strain is misleading in calculus involving natural logarithms.

The writer hopes to publish shortly an account of his own investigation and an analysis of the work of others, dealing with the subject from a different viewpoint, which he believes will explain many important phenomena hitherto misinterpreted or inexplicable.

MR. A. B. WATTS,* B.Sc. (Eng.), and DR. HUGH FORD,* Wh.Sc.: The author's initial assumption that at 60% reduction in height the stress-strain curves for the eight materials tested by Cook and Larke are already approaching a limiting value S_∞ is open to considerable doubt. The curves themselves, with the exception of that for cupro-nickel, do not convincingly demonstrate a tendency to become asymptotic to any particular value, and in the light of other considerations the inference is that they certainly do not.

Consider first the curve of octahedral shear strain plotted against percentage reduction in height (Fig. F).



Values.

Reduction in Compression, %.	Octahedral Shear Strain.	Reduction in Compression, %.	Octahedral Shear Strain.
10	0.145	60	1.112
20	0.2985	70	1.438
30	0.466	80	1.91
40	0.650	90	2.88
50	0.861	100	∞

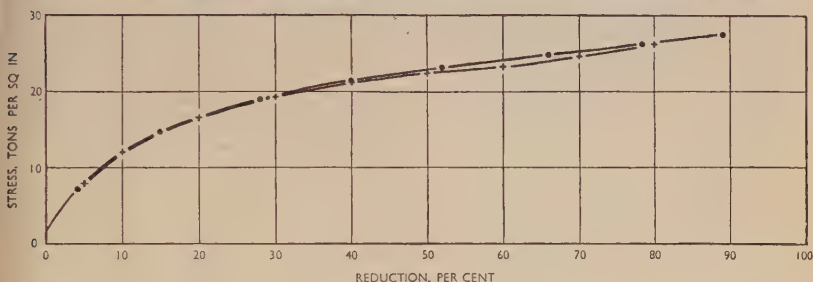
$$\gamma_{\text{oct.}} = \frac{2}{3} \sqrt{(e_1 - e_2)^2 + (e_2 - e_3)^2 + (e_3 - e_1)^2}$$

FIG. F.

From the curve it can be seen that between 60 and 85% reduction the value of $\gamma_{\text{oct.}}$ is approximately doubled. As it must be admitted that $\gamma_{\text{oct.}}$ is a far more fundamental conception of strain than percentage reduction in height, there would appear to be ample opportunity for the material to work-

* Imperial College, London.

harden beyond its condition at 60% reduction. The author does in fact agree that higher values of yield stress exist above the theoretical maximum he has calculated, and attributes them to non-homogeneous conditions. Nevertheless, the limiting value of yield stress obtained by any method of straining must be the same, whether conditions are homogeneous or not. Considering copper alone, we find that the maximum value of yield stress obtainable by cold working is in excess of 30 tons/in.² Swift* gives this value at 200% extension of electrolytic copper, and Ford† reaches it with high-conductivity copper rolled to 90% reduction in thickness, the material being tested in tension. Using the Cook and Larke technique at Imperial College, the stress-strain curve for high-conductivity copper has been determined with reasonable certainty up to 70% reduction. Polished, lubricated platens were used for the tests, not rough platens as suggested by Cook and Larke in their original paper.



KEY.

- Ford's curve for rolling (converted).
 —+— Homogeneous compression (Cook and Larke method).

Values.

Rolling (Ford).		Homogeneous Compression.	
Reduction, %.	Stress, tons/in. ²	Reduction, %.	Stress, tons/in. ²
4.2	7.2	5	7.90
15.0	14.7	10	11.95
28.0	18.95	20	16.60
40.0	21.45	30	19.45
52.0	23.2	40	21.25
66.0	24.8	50	22.50
78.4	26.25	60	23.25
89.0	27.5	70	24.70
...	...	80	26.25

FIG. G.

Fig. G shows this curve, together with the curve obtained by Ford in rolling. The latter, originally plotted to a percentage reduction in thickness base, has been converted to the same base as the homogeneous compression curve. The criterion for the conversion was equality of octahedral shear strain, rolling being considered as allowing no lateral spread, conditions being those of plane strain. A definite upward trend of both curves beyond 60% is noticeable, and agreement is very good.

In an effort to establish a homogeneous compression curve for high-

* H. W. Swift, *Metal Ind.*, 1940, 56, 127.

† H. Ford, *Proc. Inst. Mech. Eng.*, 1948, 159, 115.

conductivity copper by another means not dependent on extrapolation, an incremental method was developed. This depends on the material being compressed in small increments (1-2% reduction), the load being removed between each. Up to 40% reduction the method gave remarkably successful results, barrelling of the specimen being almost completely eliminated. It is considered that the method gives the closest approach to homogeneous compression by direct means yet obtained. Unfortunately, it was found that barrelling occurred beyond 40% reduction, and the desired effect, namely a complete homogeneous compression beyond 60% reduction, was impossible to obtain. The agreement between the incremental curve and the Cook and Larke method served to show that the extrapolation they used of $\frac{D}{H} \rightarrow \infty$ does

give a close approximation to the ideal conditions of homogeneous compression. Actually, the incremental curve was below the extrapolated, but this is exactly what would be expected from the shape of a stress-strain curve for work-hardened material springing from within the envelope formed by the curve for annealed material. The significance of this will not be discussed here, but it is mentioned because the inference that the Cook and Larke method gives too high a curve (p. 544) must be discounted. Finally, the incremental curve did not exhibit any tendency to reach a limiting value in the neighbourhood of 20 tons/in.² at 60% reduction.

In conclusion, the question may be viewed from the hardness angle. A small specimen of high-conductivity copper was compressed to 94% reduction, and had a hardness number (diamond pyramid) of 138. This may be compared with the maximum obtained by complex straining, namely 140. Using values given by Dr. Voce* in a previous paper showing the connection between diamond pyramid hardness and true stress, these values of 138 and 140 show a stress of something in excess of 30 tons/in.² Brinell tests on this material fully work-hardened can be converted to yield-stress values by using the expression given by van Iterson,† $p = 2.571 S_0$, where p is the contact pressure and S_0 the yield stress under axial load. This also gives values in excess of 30 tons/in.²

This evidence, and the general experience of tensile and other tests on high-conductivity copper, shows clearly that values of yield stress very much in excess of the $S_\infty = 23.8$ tons/in.² given by the author as the limiting stress at infinite strain, can be obtained. The fact that the strain was caused non-homogeneously cannot cause this difference, because at *infinite* strain the material must have developed its maximum strength irrespective of the intermediate mode of straining. Moreover the plot of stress against reduction in Fig. 2 of the paper does not suggest that with copper or any of the other materials a limiting stress has been reached at 60% reduction.

The AUTHOR (*in reply*): As the paper was written primarily with the object of stimulating discussion and research it is gratifying that such valuable contributions have been forthcoming in so short a time.

Special thanks are due to Dr. Cook for outlining so clearly the directions in which the proposed relationship might be used in practice.

I entirely agree that it would be a great advantage if it were proved that a homogeneous tensile curve does in fact correspond to a basic compression curve. The evidence on this point given in the paper is slight, and a small amount of further work indicated that, while both homogeneous tensile and compression curves on the same material obeyed the proposed relationship, the constants S_∞ and k , and in a lesser degree S_0 , were not quite the same in the two types of test. This may, however, have been due to difference in

* E. Voce, *Metal Treatment*, 1948, 15, (54), 53.

† F. K. van Iterson, "Plasticity in Engineering", Chapter 15. London: 1947.

the experimental conditions, for the effects of such variables as strain rate and temperature have not been studied.

While I concede to Dr. Cook that the final constant stress can only be established by an experimental determination of the whole curve, I should like to point out that, if the equation be accepted, this final stress can be ascertained from a relatively small portion of the curve by the methods described in the paper. In particular, if the initial stress and co-ordinates of the necking point are known, the index and final stress can be found with the help of Fig. 3 of my paper. Even for a compression test, the point corresponding to the maximum load in tension (necking point) can readily be located by plotting stress against strain ratio and drawing the tangent from the origin to the curve so obtained.*

That Mr. Palm and Dr. van Wijngaarden should have arrived at an identical equation by inductive reasoning and should have found it to accord with experimental observations is exceedingly encouraging. Possibly their view of the mechanism of strain-hardening is somewhat over-simplified, for polycrystalline metals can only be regarded as homogeneous and isotropic in a statistical sense, and by no means all the crystals will be oriented with glide planes parallel to the plane of maximum resolved shear stress. Nevertheless, Mr. Palm's contribution enables the equation to be visualized in terms of a mechanical model, and this is an obvious advantage.

I have never lost sight of the fact that, if valid for uni-axial stress, the equation should be applicable to, and should explain, tri-axial stresses such as those involved in necking. I am glad to find that Mr. Palm corroborates this view, and welcome the guidance he gives as to how it can be done.

I purposely refrained from naming the constants of the equation and contented myself with descriptive phrases. However, I am happy to accept Mr. Palm's suggestions of *asymptotic stress* and *characteristic strain* for S_∞ and k , respectively, it being obvious from equation (9) that k is in the nature of a logarithmic strain. It seems natural to refer to S_0 as the *plastic yield point*, or alternatively as the *initial or threshold stress*.

Since receiving Professor Mazzoleni's communication I have constructed a diagram for his equation similar to Fig. 3 of my paper, and with its help have compared his equation with my own in the light of the experimental evidence used by both of us. In every case his equation gives insufficient curvature between the initial stress and necking point, and too sudden a bend afterwards. This is well illustrated in the accompanying reproduction of Cook and Larke's homogeneous compression curve for cupro-nickel, for which there is no ambiguity about the three fixed values, initial stress, necking point, and asymptotic stress (Fig. H). In the insert to the diagram the points should, according to Mazzoleni's equation, lie on a straight line. They patently do not, and should be compared with curve *C* in the upper part of Fig. 2 of my paper. In the paper to which he refers, Professor Mazzoleni took into consideration points above the necking stress in tension where deformation was no longer homogeneous. This led to a fictitious straightening of the upper parts of his logarithmic plots at the expense of the lower parts, where there was much discrepancy between his calculated curves and the experimental data.

It appears, therefore, that there is a fallacy in the premises on which Professor Mazzoleni based his equation, but if we accept his second hypothesis, namely that the true stress is a measure of the potential energy of work-hardening, my equation (12) can be interpreted to signify that the

* A more precise method of locating the necking point, based on the fact that $(dS/de)_m = S_m$, has been suggested by Mr. Palm. When curves of S and $(\partial S/\partial e)$ are plotted against logarithmic strain on the same diagram, they intersect at roughly 90° , and thus define the necking point with great exactitude.

instantaneous rate of change of strain energy with respect to strain is proportional to the available capacity for such energy.

Mr. Polakowski draws attention to the universality of my equation, a point which I had in mind when I credited it with "a more rational significance than most of those advanced previously". I agree that it represents a manifestation of the "law of organic change", but whether the quantity that changes with deformation is the number of atoms remaining "unknotted" (Palm), or the capacity of the material for latent potential energy of work-hardness (Mazzoleni), or merely the capacity for stress (Voce), seems to be more a matter of terminology than anything else. Probably they are basically synonymous expressions dependent upon the increasing resistance which the material offers to further deformation as its glide planes and similar inherent possibilities of deformation are used up.

I much appreciate Mr. Polakowski's application of the Lagrange extrapolation method to the data given in Fig. 14 of Cook and Larke's paper,

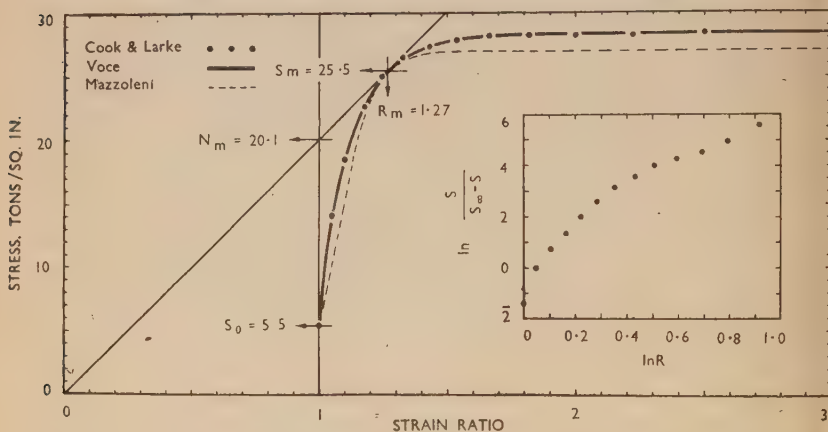


FIG. H.

and am glad to note that this results in a value of S_∞ for copper approximating to that which I deduced.

In the paper referred to by Mr. Polakowski, Siebel and Pomp determined the stress-strain curves for several materials both in tension and under compression between conical pallets. Up to the necking stress their tensile curves are not at variance with my equation, but their compression curves are entirely different. I believe this to be due to their use of conical pallets for, though this procedure can be adjusted to compress the cylinders without bulging, the strain near the axis of the specimen differs from that near the walls, since the same diminution of length is operative over a different length. Thus the process, which superficially appears to result in homogeneous deformation, gives in reality a form of compensated inhomogeneity, and agreement with my equation is hardly to be expected. As pointed out by Mr. Polakowski, the curves obtained by Körber and Müller using their indirect method, do not conform to the equation proposed in my paper. These curves refer to necked tensile specimens where deformation is far from homogeneous.

Mr. Polakowski seems at first to have missed the point of my remarks concerning the junction of the rectilinear and curved portions of MacGregor's diagrams, but in a later paragraph to have grasped it. Whenever true stress is plotted against logarithmic strain, as in MacGregor's work, the slope at

the necking point must necessarily be numerically equal to the true stress at the maximum load. As explained in the Appendix to my paper, this is an incontrovertible requirement of geometry and makes no assumptions regarding the method of testing or homogeneity of deformation. Now MacGregor claims that his experimental curves are straight lines *from the necking point onwards*. That they approximate to straight lines is evident from his experimental data, and is, I think, a more or less fortuitous consequent of inhomogeneity of deformation at the centre of the neck where MacGregor took his measurements. But that such straight lines commence *at the necking point* is impossible unless there is a sharp change of direction or discontinuity, because their slope is other than that demanded by geometry. Rather than assume such discontinuity, for which there is not the slightest evidence, I prefer to think that MacGregor's straight lines may be tangential to the curves at points slightly beyond (*not* "far beyond") the necking point, and that deformation remains sensibly homogeneous up to such tangential junction.

On this basis, the limits of homogeneous deformation for MacGregor's curves quoted in my paper are found from equation (12) and (9) to be as shown in Table A.

TABLE A.

Material.	Fig. No.	Necking Point.		Slope of MacGregor's Straight Line per Unit Logarithmic Strain, tons/in. ²	Calculated Limit of Homogeneous Deformation.	
		True Stress and Slope per Unit Logarithmic Strain, tons/in. ²	Logarithmic Strain.		True Stress, tons/in. ²	Logarithmic Strain.
Annealed Mild Steel No. 1 .	7	30.2	0.207	21.7	31.1	0.243
Annealed Mild Steel No. 4 .	7	28.0	0.199	19.3	29.0	0.244
Annealed 0.95% Carbon Steel .	7	48.4	0.174	40.6	49.2	0.188
Annealed Low-Carbon Steel .	8	35.8	0.213	24.8	37.1	0.256

The fact that the limit of homogeneous deformation seems to be distinctly greater than the necking point encouraged me to suggest the experiment described on p. 551.

Mr. Watts and Dr. Ford are critical of the fact that yield points and hardness values above those corresponding with the asymptotic stress are obtainable. In the light of the mathematical analysis given by Mr. Palm, I would suggest that the proposed equation may be concerned primarily with slip within the crystals, the asymptotic stress being attained when all the possibilities of slip under the prevailing stress system are exhausted. If this is true, the equation should provide a means of differentiating between the work-hardening due to primary slip and that attributable to the movement of crystal blocks and consequent re-introduction of slip, as described by Cook and Richards.* It is significant that, taking $S_0 = 2.5$ tons/in.², $S_\infty = 23.5$ tons/in.², and $k = 0.21$, the equation fits the curve given by Mr. Watts and Dr. Ford very well up to about 50% reduction, at which stage, according to Cook and Richards, deformation can no longer take place by the normal slip mechanism.

I wish to apologize for mis-spelling the name of Ludwik.

* M. Cook and T. Ll. Richards, *J. Inst. Metals*, 1940, 66, 6.

DISCUSSION ON PAPER BY DR. MAURICE COOK AND DR. T. LL. RICHARDS: "OBSERVATIONS ON THE ANNEALING CHARACTERISTICS OF AN ALUMINIUM-COPPER-MAGNESIUM ALLOY." *

(*J. Inst. Metals*, this volume, p. 583.)

DR. H. SUTTON,† F.R.Ae.S., F.I.M. (Member): Dr. Cook and Dr. Richards have chosen a very intriguing subject. They have themselves done a great deal of technical investigation, and they have also a wealth of industrial experience of the annealing of alloys similar to the one on which they have carried out this very interesting work.

The authors are concerned with annealing problems ranging from the most scientific features up to full-scale economic production, so that they are very fortunately situated in having a good knowledge of both sides of the matter.

It has been realized by metallurgists for some considerable time that worked metals can be softened at temperatures lower than those at which recrystallization can be observed by the methods more commonly employed to detect it, and also that very pure metals in the cold-worked state show unmistakable evidence of recrystallization at quite low temperatures.

The authors state that recrystallization effects are absent when their samples are heated to temperatures as high as $280^{\circ}\text{C}.$, whereas Hutchison‡ has recently described some very pretty experiments in which aluminium of high purity, cold worked slightly and examined at liquid-air temperatures, showed line-broadening, and after returning to normal temperature did not show those effects. Pure aluminium showed recrystallization at normal temperature after over-strain at the low temperature. Observations have been made on recovery at normal temperature in other pure metals, e.g. copper.§ I wonder whether present X-ray methods are quite sensitive enough to reveal the first stages of recrystallization.

The results of the present work link up very nicely with the general view of age-hardening following heat-treatment put forward by Farnham, Jackson, and O'Neill|| many years ago in this country. That work emphasized very strongly the identity of effect of strain-hardening by direct mechanical means and of age-hardening following the conventional solution heat-treatment.

The changes which occur in the lower ranges of annealing temperatures are particularly interesting. An observation which intrigued me very much was one due to Niemann and Stephenson¶ on a brass. They found that the internal friction, or such amount of it as is introduced by cold work in a brass, was removed by low-temperature annealing at temperatures far below the temperature of recrystallization. There are those here who know more about

* Discussion at the Annual Autumn Meeting, Cambridge, 16 September 1948.

† Deputy Director of Research and Development, Aircraft Materials, Ministry of Supply, London.

‡ T. S. Hutchison, *Nature*, 1948, **162**, 374.

§ J. L. Miller, L. C. Bannister, and R. M. Hinde, *Nature*, 1946, **158**, 705.

|| H. O'Neill, J. F. B. Jackson, and G. S. Farnham, *Phil. Mag.*, 1933, [vii], **16**, 913.

¶ F. Niemann and S. T. Stephenson, *Phys. Rev.*, 1942, [ii], **62**, 330.



FIG. A.—Deep-Drawn Samples and Transmission X-Ray Diagrams of Aluminium Sheet Annealed at (a) 400°C ., and (b) below 275°C .

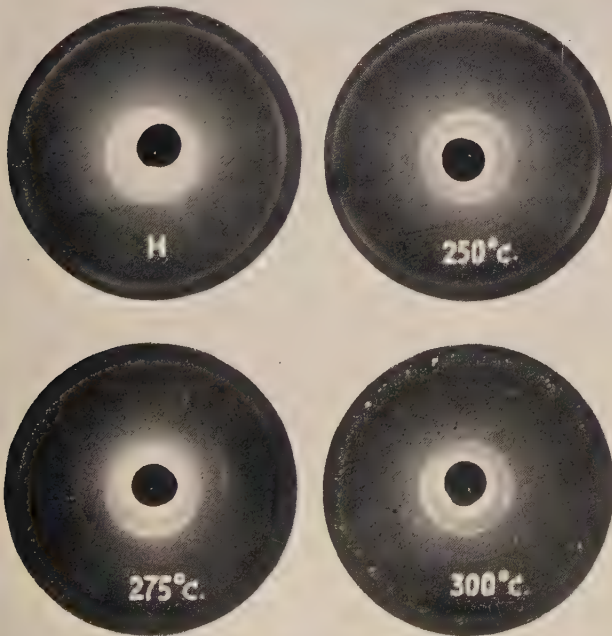


FIG. B.—X-Ray Diagrams of Anticorodal Annealed at Various Temperatures.

[To face p. 772.

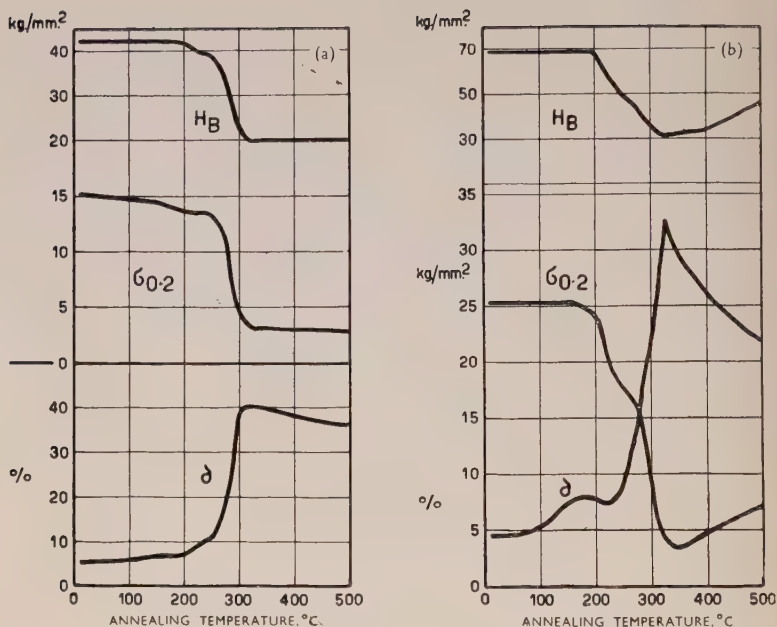


FIG. C.—Isochronal Annealing Curves for (a) Pure Aluminium, Cold Worked 90%, and (b) Anticorodal, Cold Worked 71%. Annealing time 4 hr.

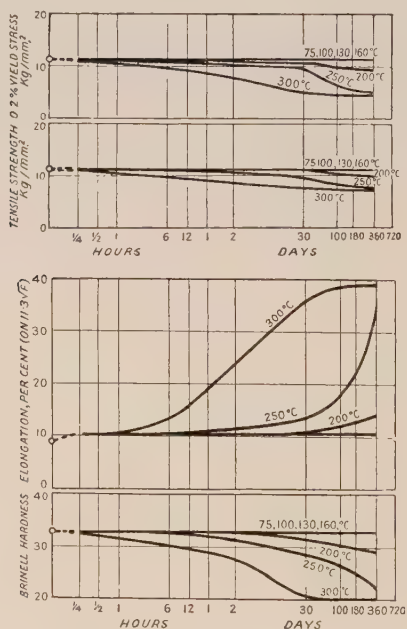


FIG. D.—Influence of Continuous Heating at Different Temperatures on: (top) Yield Point and Tensile Strength; (bottom) Elongation and Hardness of Pure Aluminium, tested at 20°C.

that subject than I do, and I should be very interested to know whether there is comparable experience on the type of alloy to which our attention is directed by this paper.

As regards the influence of other materials on crystal growth and recrystallization, speakers, discussing another paper, have referred to different fields. It is clear, however, that in these aluminium-copper-magnesium alloys, the basis of which is aluminium, which we know can be made to recrystallize, or at any rate to show substantial grain growth at normal temperature when in a high state of purity, the special elements additional to aluminium which are present in this commercial alloy must have powerful effects. It seems to me that that must be considerably to the benefit of the commercial material. I was interested some time ago to read some observations by Bassi,* working on material similar to that used by the present authors. Bassi claimed that the grain-size of the final product, after a fairly intricate technical history, was equal to that of the original. That was after a complicated technical handling programme, rather similar to some of those used by Dr. Cook and Dr. Richards. Bassi associated the effects with precipitated copper aluminide particles, and made a case for controlling crystal growth in this class of alloy by the precipitation of copper aluminide in particular conditions. Perhaps the authors would like to comment on this. I believe it is a fact which must now be accepted that the manganese-aluminium constituent in these materials can have a powerful effect on the grain growth.

The first stage of recovery or the first stage of annealing, to which the authors direct special attention, has, of course, been studied by many other investigators. A feature of this first stage which may become of interest to engineers and others who have to use these materials is the effect on the yield stress of the materials. The sheets which emerge after the techniques commonly employed have to be used for various engineering purposes, and in preparing the pieces for application it is common to apply small amounts of working. For aircraft work we have to bend the sheets; in other industries also it is necessary to make channels, and so on. When the sheets are worked, it is found that an effect known to metallurgists as the Bauschinger effect arises, whereby the "elastic limit" or the proof stress is lowered in one sense by over-strain in the other sense; that is to say, tensile over-strain will increase the tensile proof stress, but lower the compression proof stress.

Such information as we have from keen students of this effect suggests that the changes of "elastic limit" or of proof stress due to over-strain in this way are largely cancelled out by quite low-temperature heat-treatments. I should be very interested to know whether the authors feel that the previous annealing history of an alloy of the type which they have studied has much significance; that is, whether the annealing history and the resulting structure have much influence on the Bauschinger effect, the importance of which is now fully appreciated by structural engineers.

A feature which interested me in Fig. 10 was the maximum rather than the minimum. The top line in Fig. 10 refers to the 8-stage reduction, and the maxima there seem to be rather variable. Are those differences of real significance?

PROFESSOR DR.-ING. A. VON ZEERLEDER † (Honorary Corresponding Member to the Council for Switzerland) (speaking on behalf of Dr. G. F. Rohner, M. Hug, and himself): This paper is of great practical importance. The deep-drawing properties of a metal are largely dependent on its annealing characteristics. Fig. A, Plate LIV, shows at the top deep-drawn samples and a

* G. Bassi, *Metallforschung*, 1947, 2, 191.

† Director, Research Laboratories, S.A. pour l'Industrie de l'Aluminium Chippis, Neuhausen am Rheinfall, Switzerland.

transmission X-ray diagram of aluminium sheet, annealed at 400° C. The metal is completely recrystallized, without preferred orientation of the crystals and, on deep drawing, gives even edges.

In the lower half of Fig. A can be seen deep-drawn samples and a transmission X-ray diagram of aluminium sheet, annealed at something below 275° C. The metal shows a high degree of recrystallization, a pronounced preferred orientation of the crystallites, and development of ears on deep drawing. This illustrates just one practical aspect of such investigations of annealing characteristics. But Dr. Cook and Dr. Richards's paper has another aspect of a more general and more theoretical importance, namely the question whether the softening of a cold-worked alloy proceeds as a one-stage or as a two-stage process; in other words, whether recovery and recrystallization are different processes or recovery is just the first stage of recrystallization. The paper provides good evidence that the aluminium-copper-magnesium alloy investigated softens by a two-stage process. The two stages are found in the same temperature ranges in the isochronal annealing curves and in the X-ray diagrams. In a previous paper* Dr. Cook and Dr. Richards have shown that the softening of pure copper also occurs as a two-stage process. It seems, therefore, that this is the general mechanism of the softening of cold-worked alloys. But in view of the fact that the independent character of recovery is not yet universally accepted it is important to show the possibility of an experimental separation of recovery from recrystallization for other alloy systems. From investigations made at Neuhausen, I can provide evidence that recovery can be separated from recrystallization with aluminium and Anticorodal also.

In Fig. C, Plate LV, are shown on the left the isochronal annealing curves for aluminium of 99.5% purity, on the right the same curves for the aluminium-magnesium alloy Anticorodal. The pure aluminium was from a continuously cast slab, hot rolled to a thickness of 10 mm. This was annealed for 6 hr. at 350° C. and then cold rolled without intermediate annealing to 1 mm. The cold work therefore amounts to 90% reduction in thickness. Samples of this 1-mm. sheet were afterwards annealed for 4 hr. at temperatures ranging from 100° to 500° C. in an air-atmosphere furnace and air cooled.

The Anticorodal was from a continuously cast slab, hot rolled to 7 mm. This was cold rolled to 1 mm., with an intermediate annealing (4 hr. at 380° C.) at 3.5 mm. The cold work in this case amounts to 71%.

Up to about 180° C. the Brinell hardness of pure aluminium remains constant, and the proof stress shows only a small decrease. From 180° to 210° C. both Brinell hardness and proof stress show a pronounced decrease which is interrupted from 210° to 250° C., where a rapid decrease sets in, which stops at 320° C. The corresponding X-ray diagrams show the first sign of recrystallization at 275° C. The diagram for 300° C. reveals almost complete recrystallization. The X-ray diagrams therefore show that the rapid decrease from 250° to 300° C. must be due to recrystallization. For the first small decrease from 180° to 210° C. the X-ray diagrams give evidence of recovery but no signs of recrystallization. It is a well known fact that recovery, which must be the cause of this initial decrease, does not manifest itself very distinctly in the X-ray diagrams of pure aluminium, because cold-worked pure aluminium itself gives sharp reflections.

I would refer again now to Fig. C, Plate LV. With the age-hardening alloy Anticorodal the two-stage character is very pronounced in the proof-stress curve, but less so in the curve for Brinell hardness. The first rapid decrease extends from 190° to 230° C., the second from 275° to 340° C. Fig. B, Plate LIV, shows some of the corresponding X-ray diagrams. The diagram

* *J. Inst. Metals*, 1947, 73, 1. ●

of the cold-rolled sheet is very diffuse. The $K\alpha_1$ and $K\alpha_2$ diffraction rings can hardly be distinguished. The diagram of the sample annealed at 250° C. shows a very different aspect. The lattice distortions, which must have caused the diffuseness of the diffraction rings in the first diagram, have disappeared. This must be due to recovery! The diagram at 275° C. is in no way different from the one at 250° C. Release of the lattice distortion has proceeded no further and recrystallization has not yet set in. The diagram for the sample annealed at 300° C. shows that recrystallization has set in, but probably not yet reached an amount of 50%. So the second rapid decrease in proof stress has, by means of X-ray diffraction, been related to recrystallization.

The independent character of recovery seems therefore well established for pure aluminium and Anticorodal. This means that, in these two cases also, the softening proceeds by a two-stage mechanism, as with pure copper and Duralumin, investigated by Dr. Cook and Dr. Richards.

There is one feature of minor importance where our findings differ to a certain extent from those of Dr. Cook and Dr. Richards. Neither with pure aluminium nor with Anticorodal have we obtained an initial increase in hardness, as Dr. Cook and Dr. Richards have for Duralumin in the temperature range 20°–160° C. On p. 586 of their paper they express the view that the occurrence of such an initial increase is quite usual and that it should be small for pure metals and greater for solid-solution alloys. The explanation which Dr. Cook and Dr. Richards give for the cause of this initial increase in hardness is in my view not very clear and convincing. I can show another curve (Fig. D, Plate LV) from the report of an investigation published in 1936 in the *Journal* of the Institute* which again shows no initial increase in hardness when pure aluminium is annealed. I think, therefore, that for a further elucidation of this phenomenon it would be valuable to know why Dr. Cook and Dr. Richards observed it with Duralumin while we did not with pure aluminium and Anticorodal.

MAJOR P. C. VARLEY,† M.B.E., T.D., M.A. (Member): This paper is a very interesting one, and, as Professor von Zeerleder has already said, it raises a number of points of general interest in regard to the recovery and recrystallization of aluminium and its alloys. I think that it is perhaps from that point of view a little unfortunate that the system chosen is such a complicated one, as it is rather difficult to separate the phenomenon of age-hardening and the solution and precipitation of the various constituents from the phenomena of recovery and recrystallization. I find that in a number of instances I am inclined to differ from the authors as to the exact interpretation of their results. Thus, the increase in definition of the diffraction rings in the X-ray photographs during the early stages of annealing is ascribed by the authors to recovery, but, although this effect has been searched for by both myself and my colleagues, it has never been observed in any sample of pure aluminium or of the simple binary alloys.

Spillett‡ states that "a careful examination of the X-ray patterns reveals that no sharpening of the blurred spots occurs either before or during recrystallization; the new crystals give rise to well-defined isolated spots randomly distributed on the Debye-Scherrer rings". In other words, if one takes a piece of aluminium with fairly coarse grain-size and rolls it down so that one can see the individual spots slightly blurred, and then anneals it by stages, those spots show no change at all until one suddenly gets the appearance of sharp new spots characteristic of the fresh crystals. Thus, it seems more probable that this increase in sharpness of the rings during annealing, and also the

* A. von Zeerleder and R. Irmann, *J. Inst. Metals*, 1936, 59, 111.

† Metallurgist, British Aluminium Co., Ltd.

‡ E. E. Spillett, *J. Inst. Metals*, 1943, 69, 160.

difference in the sharpness of the rings obtained from metal rolled with a recrystallizing or with a non-recrystallizing anneal, is due to local lattice distortion due to the presence of copper and magnesium atoms in the solid solution.

On the other hand, although it appears at first sight that the peculiar step or discontinuity at about 280°C . in the isochronal annealing curves, which the authors ascribe to the change from recovery to recrystallization, might well be due to some form of age-hardening, I have been entirely unable to account for it, even qualitatively, on these lines. In Fig. E, curve *A* is an isochronal annealing curve for a sample of commercial-purity aluminium, and there is no step on it. In that I differ from Professor von Zeerleder; although I have done many of these determinations, I have been unable to find any trace

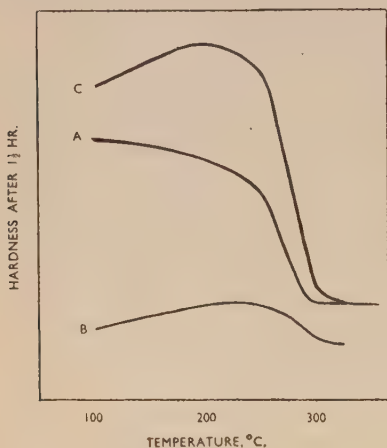


FIG. E.

of it with pure aluminium. Curve *B* is an isochronal age-hardening curve for a sample of an aluminium-copper-magnesium alloy of approximately (but not exactly) the same composition as that used by the authors, quenched from 380°C . Curve *C* has been constructed by adding to curve *A* three times the observed age-hardening effect in curve *B*. The addition of three times was some attempt to allow for the acceleration of age-hardening produced by cold work, and another example of the difficulties of sorting out the two effects. It will be seen that the first part of that is very similar to the curves found by the authors; in other words, it shows an initial hardening; but there is no step produced by this summation, and the only way in which we can get a step on the curve in the

right place is by making two unjustifiable assumptions. The first is that the maximum on the age-hardening curve occurs at about 300°C . instead of at about 250°C ., which is extremely unlikely, and the second is that recrystallization of the aluminium matrix is actually complete before the step occurs, which the authors have shown—and I am sure that they are right there—is not the case.

It would therefore appear that that step cannot be simply explained on an age-hardening basis.

In a paper shortly to be published by the Institute,* I have shown that for aluminium of commercial purity recovery and recrystallization take place continuously at all temperatures, the one process merging into the other with no detectable discontinuity, other than the appearance of new crystals, in either the isothermal or the isochronal annealing curves, and with no apparent change in the activation energy of the process.

Similar results have been obtained with some of the simple binary aluminium alloys, but preliminary experiments with an aluminium-copper-magnesium alloy of a composition similar to that studied in the paper by Dr. Cook and Dr. Richards gave a well marked step of the type under discussion, so that there is a difference of behaviour in this respect between the aluminium-copper-magnesium alloys and the simpler ones.

I was interested to read on p. 593 of the present paper of the effect of

* *J. Inst. Metals*, 1949, 75, 185.

intermediate anneals on the recrystallization temperature, and the explanation given. A similar effect is observed in aluminium of commercial purity, and here it seems possible to build up an explanation based on the varying amounts of silicon in solid solution, though I am not entirely satisfied that this is the whole story.

Finally, I should like to ask the authors to clarify their explanation of the shape of the curves in Fig. 13. On p. 589 they appear to consider that the shape of curve (b) is explained by age-hardening and its acceleration by cold work, whereas on p. 591 they appear to ascribe it to a change in the mechanism of deformation. It would be helpful if they would say which explanation they believe to be the correct one.

MR. R. W. CAHN,* B.A. (Student Member): An interesting point, which may give a clue to one type of recovery, appears from Fig. 14 of the paper. In curves A_1 , B_1 , and C_1 , the amount of recovery—i.e. the relative decrease of hardness—increases as the original hardness increases. Specimen C_1 was the hardest, because its last rolling reduction was the greatest, and it has recovered by the greatest amount, while A_1 recovered the least. This is analogous to the results of experiments on the electrical resistance of cold-drawn tungsten wires as a function of time of annealing †; this property recovered down to limiting values which approximated the more nearly to those found with recrystallized material, the higher the temperature of annealing. Other workers have noted corresponding effects.

I should like to put forward a speculation as to the explanation of these facts. I have done some experiments, not yet published, with bi-crystals separated by a macroscopically straight boundary. They were stressed in shear at high temperatures, and the displacement of previously made transverse marks was measured. It proved that the rate of creep became almost zero after a while, and the total displacement increased with the value of the applied stress. This decrease in creep rate is presumably due to the progressive increase in local stresses at microscopical irregularities in the boundary. Now it is well known that most of the local stresses in a deformed metal are localized near the boundary, and it may be that what happens in the authors' alloy is that the different states of stress in adjacent grains lead to a very slight relative displacement at the boundaries; this may be sufficient to relieve some of the highly localized stresses and therefore to reduce the measured hardness. Such a displacement is limited (a) by the geometry of the boundaries, (b) by the value of the stresses leading to the displacement. The latter must mean that the motion stops when the stresses have been reduced by a certain amount; similarly, the recovery of the properties depending on the state of stress will also stop.

If such a process does in fact take place in polycrystalline alloys, it might be possible to show it up by scribing a fine net of lines on a cold-worked and polished specimen before annealing, and examining it afterwards. However, the effect might be too small to be discovered by this method.

DR. T. LL. RICHARDS (*in reply*): I agree with Major Varley that the aluminium-copper-magnesium alloy is not the ideal material for studying such processes as recovery and recrystallization. The work described in our paper represents part only of a technological investigation on a commercially important material, and has been published because of its special interest in connection with these processes. Alloying additions undoubtedly have considerable influence on annealing characteristics. Recovery, however, can

* Atomic Energy Research Establishment, Ministry of Supply, Harwell, Berks.

† W. Geiss and J. A. M. van Liempt, *Z. Metallkunde*, 1926, 18, 216.

occur in pure metals, and in the photographs reproduced in the note by Hutchison, referred to by Dr. Sutton, there appears the same type of evidence which Dr. Cook and I regard as indicative of recovery, namely diffuse rings characteristic of the distorted structure of material cold worked at liquid-air temperature, and sharp, well defined rings characteristic of material recovered on standing at room temperature. In addition, sharp diffraction spots appear in the photograph of the specimen which has stood at room temperature after cold working at liquid-air temperature, which is evidence that self-annealing or recrystallization is also taking place. I believe that the recovery effect is not observed with super-pure aluminium deformed at room temperature because it is too rapid at such temperatures. As Professor von Zeerleder has already shown, recovery can be observed in commercially pure aluminium, for the high-order diffraction rings of material cold rolled at ordinary temperature are diffuse and increase in definition when the metal is annealed at more elevated temperature. It would thus appear that the presence of impurities retards the recovery process.

We made no attempt to provide a complete list of references, for the literature on this subject is now so extensive that a comprehensive list would take far too much space. I would recommend anyone who wants fuller references to turn to the works of Professors W. G. Burgers and C. S. Barrett.

With regard to Dr. Sutton's comments on our Fig. 10, I do not think any significance should be placed on the variation in the hardness values at the maxima. The results recorded in this Figure were actually obtained in the course of check tests to ensure that our material was in the condition required at each stage of the processing. The results are presented since they illustrate a point we wish to make. It was because we were not quite satisfied with the accuracy of the data in Fig. 10 that the experiment, the results of which are plotted in Fig. 13, was undertaken under more controlled conditions.

Professor von Zeerleder's observations are a striking confirmation of our own views. Like him, we are not certain of the cause of the initial hump in the annealing curves, but we have associated it with ageing since the initial increase in hardness is only observed for specimens of the alloy in an ageing condition. I should add that this initial rise in ordinary-isochronal annealing curves is generally very small for pure metals, but quite considerable for solid-solution alloys such as 70 : 30 brass. Another striking example is the strain-ageing effect in mild steel. I can do no better than refer to the explanation of these effects which has been put forward by Cottrell and Bilby.*

The considerable effect which a small amount of impurity can have on a metal is not surprising when one realizes that if there is only about 0.01 at.-% of a foreign element uniformly distributed in a metal then every twentieth atom in any direction will be an atom of the foreign element. On cold working, dislocations are developed in the crystal lattice, and provided that the relative sizes of the matrix and solute atoms and the diffusion coefficient of the latter are favourable there is appreciable diffusion of the foreign atoms to the region of the dislocation, causing an increase in hardness.

With regard to the curves A, B, C in Fig. E given by Major Varley, if the alloy he examined is similar to ours, I am a little surprised that he did not observe two distinct softening steps, since they are so clearly separated in our curves that there can be no doubt at all about their existence. I notice that the temperature at which softening is first observed in Major Varley's curves is almost in the range where we observe the second step, so it is possible in his case that the two stages so overlap that two distinct steps in softening cannot be demonstrated.

We shall examine with interest Mr. Cahn's suggested explanation of the mechanism of the recovery process.

* A. H. Cottrell and B. A. Bilby, *Proc. Phys. Soc.*, 1949, [A], 62, 49.

The AUTHORS (*in further, written, reply*): In reply to Dr. Sutton's question regarding sensitivity of X-ray methods, there seems to be little doubt that the back-reflection X-ray technique is not only sufficiently sensitive to reveal the first stages of recrystallization, that is, the appearance of new crystals, but also, in favourable circumstances, as shown in this paper, the changes associated with recovery, which occur prior to recrystallization. During recovery, changes in properties occur in these cold-worked alloys, which are accompanied by a structural change, the evidence for which is a sharpening of X-ray diffraction rings.

There is, as far as we know, no published information on the correlation of variation of internal friction with the structural changes occurring in alloys of the type investigated.

It is generally recognized that grain-size in the aluminium-copper-magnesium alloys varies but little with processing conditions, and in this respect the alloy differs markedly from aluminium, which is capable of giving a large range of grain-sizes according to the rolling and annealing procedure. We agree with Bassi's conclusion that the precipitation of copper aluminide from solid solution affects the grain-size as well as other characteristics of the alloy referred to in the present paper, and this matter will be dealt with in a forthcoming publication. We also endorse the view that manganese probably has an important effect on grain growth, although we have made no systematic observations of its effect in this particular alloy.

The effects of variations or differences in earlier thermal treatments are largely cancelled out by the final heat-treatment operation at about 500° C., and it would not seem, therefore, that they could have much influence on the Bauschinger effect, but we have not made any actual observations on this point.

It is not possible to generalize, on the evidence available, concerning the nature and causes of the initial hardening which is a common feature of the annealing curves of many materials, but so far as alloys of the type investigated are concerned, this would, as the evidence in the paper suggests, appear to be associated with ageing. Whether or not such hardening occurs, and the extent of it, is clearly related to the thermal and mechanical treatments to which the material has been previously subjected, as the curves in Fig. 14 show. Although, like Professor von Zeerleder, we have not observed any initial hardening in annealing curves for aluminium, we would expect that in an age-hardenable type of alloy like Anticorodal, it could occur, and, in fact, we have noted it to the extent of some 16 diamond pyramid hardness numbers, on an annealing curve of this material previously solution heat-treated at 540° C., quenched, and cold rolled with a reduction of 50%.

In further reply to Major Varley's points, we would first emphasize that the X-ray transmission technique is quite insensitive for showing decrease in lattice distortion on recovery, so that it is not surprising that he and his colleagues failed to observe the effect if they, like Spillett, used this technique. Spillett's observations, from X-ray transmission photographs, are not contrary to, but completely in accord with, our own. We also find that there is no change in character of transmission photographs on annealing the cold-worked metal, apart from the appearance of spots, due to the growth of new crystals. Evidence for recovery was provided by back-reflection photographs only. We agree that the increase in sharpness of the diffraction rings during annealing is bound up with the effect of solute elements, and, in fact, the presence of solute elements modifies the first softening or recovery stage, as described in the paper.

It is not surprising that Major Varley cannot account for the discontinuity shown by the annealing curves on the basis of ageing, for there does not seem to be much evidence to support this view. The absence of any similar dis-

continuity on annealing curves of pure aluminium is probably to be accounted for by the fact that in this metal recovery occurs at room temperature.

Regarding the shape of the curves in Fig. 13, there is, in fact, no inconsistency in the explanations which have been offered. It is the relative displacement of curve (a) and (b) that is explained by age-hardening, while the shape of curve (b) is accounted for on the basis of a change in the mechanism of deformation with increasing degree of cold working. The shape of curve (b), however, is to some extent influenced also by age-hardening effects, but for curve (c) ageing is complete at each stage, and is no longer a factor in determining shape. The shape of curve (c), therefore, can only be explained by a change in the mechanism of deformation.

As Mr. Cahn points out, changes in the electrical resistance of cold-drawn tungsten on isothermal annealing may throw light on the process of recovery. In this connection, the observations of Kornfeld * are of interest. He showed that the yield point of strained aluminium crystals decreased, on isothermal annealing, to a steady value by an amount proportional to the degree of initial work-hardening.

* M. O. Kornfeld, *Physikal. Z. Sowjetunion*, 1934, 6, 39.

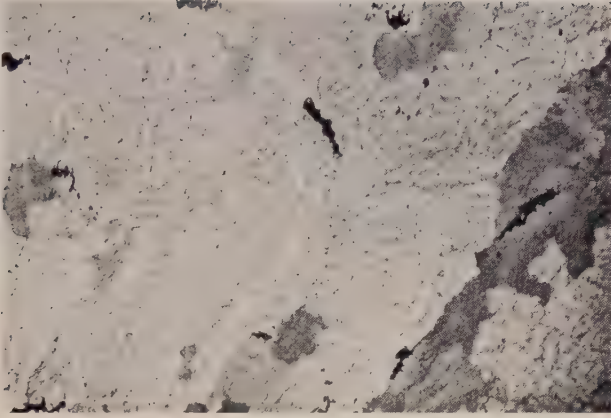


FIG. B.—Peripheral Zone of Ingot of 500 mm. dia. Hydrogen content of liquid aluminium: $0.150 \text{ cm}^3/100 \text{ g.}$ $\times 15$.

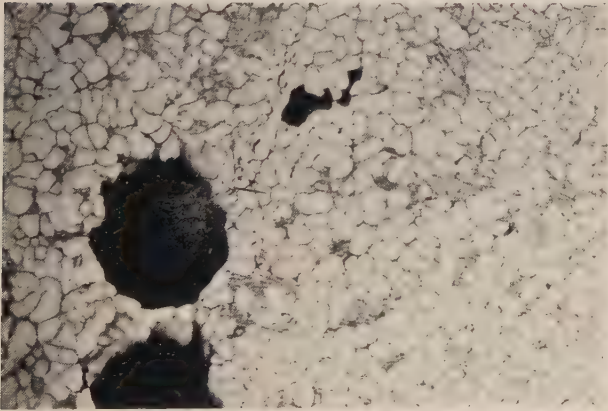


FIG. C.—Central Zone of Ingot Shown in Fig. B. $\times 15$.

[To face p. 780.]

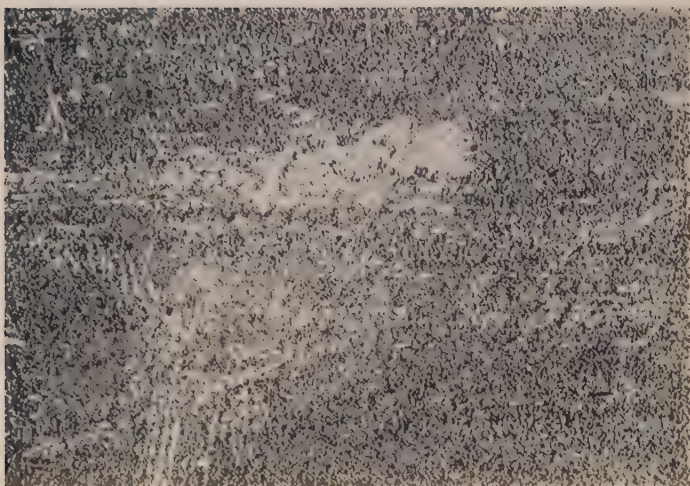


FIG. D.—Effect of Heating at 620° C. for 1 day on Duralumin Sheet (Cu 2.26, Mg 0.61, Mn 0.79, Si 0.55, Fe 0.57%). Hydrogen content ≤ 0.010 cm.³/100 g. $\times 3.5$.

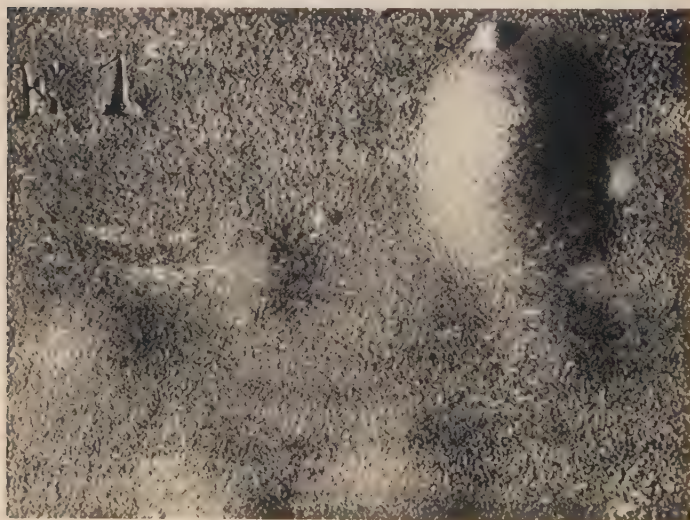


FIG. E.—Effect of Heating at 620° C. for 1 day on Duralumin Sheet (Cu 2.27, Mg 0.65, Mn 0.79, Si 0.43, Fe 0.49%). Hydrogen content 0.045 cm.³/100 g. $\times 3.5$.

CORRESPONDENCE ON PAPER BY DR. C. E. RANSLEY AND MR. H. NEUFELD: "THE SOLUBILITY OF HYDROGEN IN LIQUID AND SOLID ALUMINIUM."

(*J. Inst. Metals*, this volume, p. 599.)

M. YVES DARDEL *: I have read with much interest the results in this paper as I have recently determined the solubility of hydrogen in solid aluminium, as well as the effect of dissolved hydrogen on the properties of aluminium alloys. As the analytical method which I have used is very different from that of the authors, I would take the liberty of comparing their results with my own, now in process of publication.

The hydrogen content was determined in the following manner †: The sample, if solid, is melted, held at a constant temperature, and the atmospheric pressure above it slowly lowered. When the first bubble appears, the temperature and the pressure are measured. If it is then admitted that the hydrostatic pressure is equal to the internal pressure in the bubble, the amount of dissolved hydrogen is given by the relation:

$$\log m = -\frac{4.702}{T} + 2.844 + \frac{1}{2} \log p \quad . \quad . \quad . \quad (1)$$

m being in $\text{cm}^3/100 \text{ g.}$ and p in mm. Hg, whence the Sieverts' constant for liquid aluminium ‡ is given by:

$$\log K_s = -\frac{4.702 \times 5}{T} + 0.238 \quad . \quad . \quad . \quad (2)$$

The relative accuracy of this method varies from about $\pm 10\%$ when the hydrogen content is greater than $0.08 \text{ cm}^3/100 \text{ g.}$ to about $\pm 20\%$ for a hydrogen content of $0.010 \text{ cm}^3/100 \text{ g.}$; but the absolute accuracy of the determinations depends then upon the values adopted for the solubility in liquid metal.

Hydrogen solubility in solid metal (aluminium, 99.5% purity) at the melting point has been determined as follows: liquid aluminium contained in an iron crucible was saturated with hydrogen, then slowly cooled in water. After X-ray examination to make sure of the absence of porosity, the hydrogen content was determined. The solubility values at lower temperatures were obtained by carrying out the determinations after an isothermal treatment of the samples in an atmosphere of hydrogen.

According to my determinations, the hydrogen solubility in solid aluminium can be represented by the equation §:

$$\log m = -\frac{4.264}{T} + 1.898 + \frac{1}{2} \log p_{\text{H}_2} \quad . \quad . \quad . \quad (3)$$

* Metallurgical Engineer, Centre de Documentation Sidérurgique, Paris.

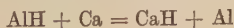
† Y. Dardel, *Metals Technol.*, 1948, 15, (8); *A.I.M.M.E. Tech. Publ.* No. 2484.

‡ Y. Dardel, *Metals Technol.*, 1947, 14, (6); *A.I.M.M.E. Tech. Publ.* No. 2247.

§ This equation has already been used by me for the calculation of the change of free energy of the reactions:



and:



It is of interest to compare both sets of data in the light of the empirical laws of the solubility of hydrogen in metals.

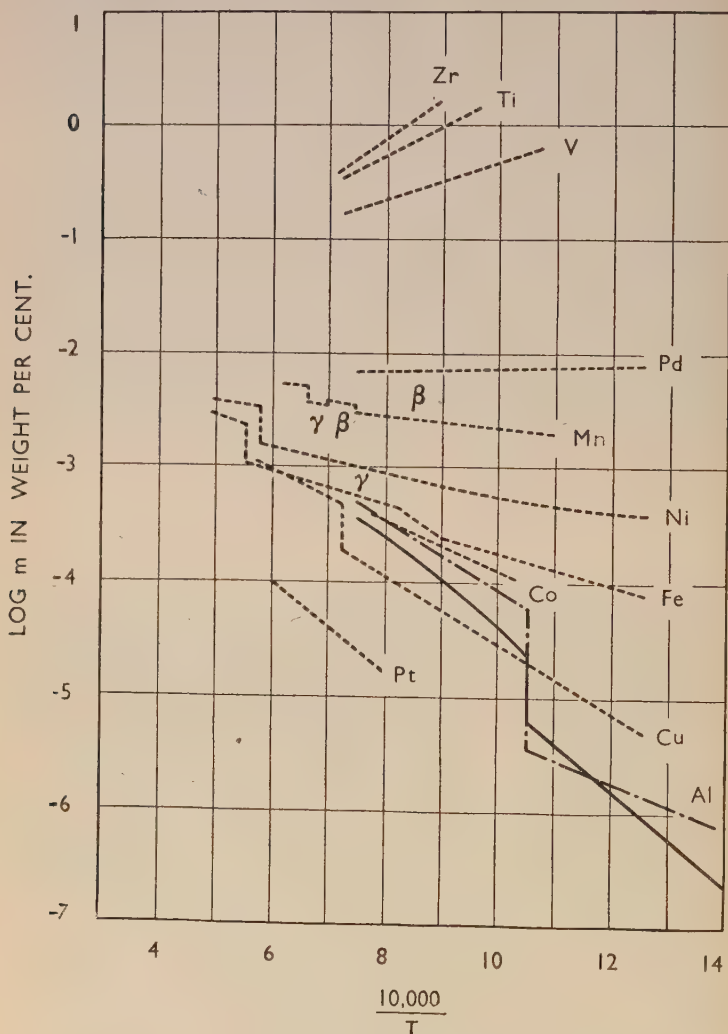


FIG. A.—The Solubility of Hydrogen in Different Metals.
 — · — Ransley and Neufeld.

Firstly, it must be pointed out that a slight error has slipped into the values reported by the authors in the first page of their paper (p. 599)—Braun in his dissertation gives $0.24 \text{ cm.}^3/100 \text{ g.}$ at 700° C. as the mean solubility and not 0.08 as has been reported. So the dispersion range of the results of the experiments by Röntgen and Braun, Röntgen and Möller, and Bircumshaw remains

constant for all the temperatures and does not diminish at high temperature as indicated by the authors.* On the other hand, the results of the authors, as well as those of Baukloh and Oesterlen, also have a small dispersion range, but with another mean value. Both mean curves are shown in Fig. A, with the solubility curves of the other metals. If the empirical law of Borelius † is to be accepted as a perfect one, it would seem that the best solubility values would lie between the two sets of data.

According to the same Figure, the solubility values which I have determined are very little different from those of the authors. However, if the hydrogen-solubility values determined by the authors for liquid aluminium are to be accepted, the solubility as determined by myself must be increased, i.e. when the solubility in liquid metal at melting temperature is equal to 0.17 cm.³/100 g. the solubility at the same temperature but in solid metal is equal to 0.058 cm.³/100 g. On the other hand, if the figure of 0.69 given by the authors is to be accepted, my determinations give then a value of 0.20 cm.³/100 g. in solid metal.

Another point to be noticed is the very high solubility ratio (19) determined by the authors at the melting point (Table A). This solubility ratio is in disagreement with all the determinations I have carried out, either on small charges in a laboratory or on big charges (up to 4 tons) in a foundry, with aluminium alloys or aluminium of 99.5% purity. Besides, the solubility ratio given by the authors seems to be in disagreement with the values found for the other metals (Table A). The solubility ratio increases with the decrease in solid solubility at the melting point, but nothing like so quickly.

TABLE A.

Metals	Mn	Fe	Ni	Cu	Al	
					Dardel	Ransley and Neufeld
Solubility ratio	1.80	2.07	2.14	2.73	2.93	19
log H ₂ (H ₂ being the solubility in the solid state at the melting point, expressed in wt.-%. pH ₂ = 1 atm.)	2.415	2.922	2.789	3.753	5.282	5.49

Like the authors, I have noticed that when pores are present in an ingot, the hydrogen content is always much greater than the solubility values; from 0.090 to 0.150 in my experiments. But I do not think that these values are attributable to a supersaturation of the material, for the hydrogen determinations which I have carried out to evaluate the solubility at the melting point were undertaken with ingots which, according to the authors, would have been supersaturated. However, the results which I have obtained in this manner agree with the determinations made on samples held in a hydrogen atmosphere at a lower temperature, and therefore not supersaturated.

I think that the increase observed is caused by the absorbed layer. Indeed, according to my experiments, it is impossible to clean a sample with a degreasing agent; samples must be turned thoroughly. When a sample contains

* See Fig. 1 of *A.I.M.M.E. Tech. Publ.* No. 2247.

† G. Borelius, *Ann. Physik*, 1927, [iv], 83, 121.

pores, many of them debouch at the surface, but as their internal surfaces cannot be clean, their presence increases the apparent hydrogen content. When a material contains pinholes, the hydrogen determinations do not give a true picture of the hydrogen content.

During freezing, hydrogen tends to be distributed according to Nernst's law. When the hydrogen content of a liquid metal is greater than the solubility in its solid state, pinholes appear at melting temperatures. The trend

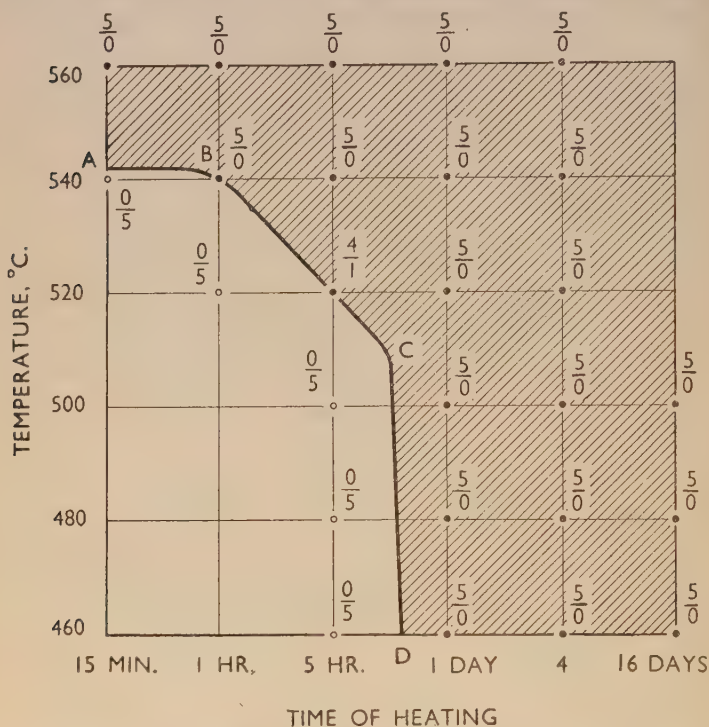


FIG. F.—Formation of Blisters on Wrought Duralumin (Cu 2.93, Mg 1.18, Mn 0.67, Si 0.47, Fe 0.46%. Hydrogen content 0.047 cm.³/100 g.)

Expressed as : $\frac{\text{number of samples with blisters}}{\text{number of samples without blisters}}$

of the hydrogen to be distributed according to Nernst's law can be seen in Figs. B-C (Plate LVI), taken from an ingot of 500 mm. dia. cast by cooling in water. The pores in the exterior part of the ingot are small and follow the shape of the dendrites; they appear when the dendrites are already grown. On the other hand, in the core of the ingot the pores are round, and therefore they appear before the growth of the dendrites; the core of the ingot has been enriched with hydrogen during the freezing of the surface.

When the hydrogen content of the liquid aluminium is less than the solubility limit at the melting point, hydrogen cannot cause pinholes during casting. But if the material contains enough hydrogen at the melting temperature, pores and blisters can always appear during heat-treatment or

welding when the internal pressure of the hydrogen is greater than the resistance of the alloy to deformation (Figs. D-E, Plate LVII).

In Fig. F the isothermal curve for pore and blister formation in a Duralumin sheet is given, in the fashion already suggested by Borchers and Castellani,* as well as by Kostron.† The form of this curve can also be roughly calculated.

Hydrogen dissolved in a grain diffuses into the internal cavities always present ‡ according to Fick's law :

$$dn = -D \cdot \frac{dc}{dx} \cdot dt \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where n = number of molecules, D = diffusion constant, and t = time of diffusion.

According to the experiments of Ham on platinum and nickel,§ as well as those of Smithells and Ransley on aluminium,|| the diffusion speed is controlled by the desorption speed, and therefore the concentration of hydrogen in a grain is independent of the co-ordinates and the equation can be integrated between 0 and t :

$$N = -D \times K_1 \times t \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Hydrogen which diffuses into a cavity in a metal is at a pressure p , given by :

$$pv = N \cdot R \cdot T \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

whence :

$$\log t = \log p - \log D + K_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

as the variation of $\log T$, being small, can be neglected. According to the experiments of Smithells and Ransley one has for pure aluminium :

$$\log D = -\frac{6.742}{T} + 0.518 + \frac{1}{2} \log P \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

When the pore appears, p is much smaller than the internal pressure in the grain and P can be deduced from equation (3). Moreover, calculations of the strength of materials show that :

$$p = K_3 \sigma_F \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

σ_F being the resistance to deformation ($K_3 = \frac{2}{3}$ when the pinhole is spherical).

One obtains then :

$$\log t = \log \sigma_F + \frac{2.478}{T} + K_4 \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

t giving the time for the diffusion of the same amount of gas in the internal cavities.

Between 440° and 510° C. the values calculated for the temperature coefficient of the line CD and those which have been measured by planimetry of the surface of the blisters are in reasonable agreement (3.536 against 3.220). The line AB corresponds to the incipient-melting temperature. The isothermal curves of pore and blister formation give then for each temperature the maximum time for which it is possible to heat the material before pores and blisters appear.

The determination of the isothermal heating curves of an aluminium alloy allows the measurement of the maximum permissible amount of hydrogen in

* H. Borchers and O. Castellani, *Metall u. Erz*, 1942, **39**, 407.

† H. Kostron, private communication.

‡ G. Tammann and H. Bredemeier, *Z. anorg. Chem.*, 1925, **142**, 54.

§ W. R. Ham, *J. Chem. Physics*, 1933, **1**, 476.

|| C. J. Smithells and C. E. Ransley, *Proc. Roy. Soc.*, 1935, [A], **152**, 706.

an aluminium alloy to be welded. In the case of welding, the material is brought to a high temperature in a very short time, so that blisters and pores appear only when the temperature has exceeded that of incipient melting. But above this temperature, the local resistance to deformation is nil and therefore blisters and pores appear as soon as the internal pressure of dissolved hydrogen is greater than one atmosphere.

The AUTHORS (*in reply*): We have read M. Dardel's contribution with great interest, and regret that the paper describing his new results has not appeared in time for us to study it before making our reply.

To deal first with a minor point which he raises, the value of 0.08 c.c./100 g., given on p. 599 as the solubility found by Röntgen and Braun at 700° C., is taken from Fig. 1 and Table II of their paper. These authors state, however, that the solubilities they observed at 700° C. were of the same order as their expected experimental error, so that little or no significance can be attached to their figure for this temperature.

The fact that the liquid : solid solubility ratio we have found for aluminium is considerably higher than the corresponding ratios for quite dissimilar elements, such as iron and nickel, does not appear to us to be surprising, nor to give any basis for questioning the validity of our results. As a matter of interest we have recently obtained additional data which confirm our liquid-solubility values quite closely, and we hope to be able to publish these in the near future. On the other hand, our own experience on the measurement of the hydrogen contents of aluminium and its alloys by hot extraction suggests that the values of gas contents deduced by M. Dardel from results on his apparatus are very much too low; for example, the results given in Figs. 2-3 of his earlier paper,* as well as those quoted in his present contribution, appear to us to be too small by a factor of at least 4 or 5. We suggest, in fact, that they are based on solubility values which are erroneous in some such proportion.

As M. Dardel points out, if the results obtained by his method are calculated on the basis of our solubility values, his value of the solid solubility of hydrogen at 660° C. then becomes rather more than 0.2 c.c. instead of 0.058 c.c./100 g. We cannot comment on this point in any detail without knowing more about the experimental procedure he adopted for his determinations, but wish to remark that a true gas content of this order (0.2 c.c./100 g.) seems to us to be very reasonable for metal saturated with hydrogen in the liquid state and frozen in the way he describes. The absence of porosity of the type shown up by X-ray examination cannot be taken as proof that the hydrogen content of the metal does not exceed the equilibrium solid solubility, since the porosity produced by a given gas content depends upon the rate of solidification of the metal; with sufficiently rapid chilling, e.g. in chill-cast bars, the hydrogen content may be as high as 0.5 c.c./100 g. without producing any very marked porosity.

We would like to make it clear that the hydrogen-content figures given in our paper are fully corrected for surface gas. The supersaturation effect observed in the porosity-hydrogen content curve cannot therefore be ascribed to this cause.

M. Dardel's results on the blistering of Duralumin sheet are very interesting, but we are unable to follow his mathematical treatment of the problem. It is very improbable that desorption is a rate-controlling process when the interface under consideration is perfectly clean, as is probably the case in the release of gas from solid solution into an internal pore. But one obviously cannot assume that desorption *is* rate-controlling and at the same time treat the rate of pressure rise in the pore as some function of diffusion.

* *Metals Technol.*, 1947, 14, (6); *A.I.M.M.E. Tech. Publ.* No. 2247.

OBITUARY.

HARRY BREARLEY.

Mr. Harry Brearley, Technical Director of Brown, Bayley's Steel Works Ltd., died on 14 July 1948 at his home in Torquay at the age of 77.

He was born in February 1871, and received his schooling at Woodside Board School, but began work at the age of 12 with Thomas Firth and Sons Ltd., as a cellar lad. Subsequently he entered the laboratory under James Taylor, to whom Mr. Brearley frequently paid tribute for his help and encouragement.

In 1901 he left Firth's in order to start a new laboratory at Kayser Ellison and Co. Ltd. While there he wrote, in collaboration with the late Fred Ibbotson, "The Analysis of Steelworks' Materials". Returning to Firth's in 1903, he was offered, in 1904, the appointment of chief chemist at their Salamandar Works, Riga, and in 1905 was appointed works manager there. While at the Salamandar Works he became interested in the production of pyrometers, and in that connection was responsible for the formation of Amalgams Company Ltd., which produces the pyrometer bearing his name.

At the end of 1907 he returned to Sheffield to design and equip the proposed joint research laboratories for Thos. Firth and Sons Ltd. and John Brown and Co. Ltd. While studying the causes of erosion and fouling in rifle barrels in 1912, experiments were carried out by Mr. Brearley which eventually led to the discovery of the stainless properties of high-chromium steels. As a result of further investigations with these high-chromium steels, the first stainless knives were made for Mr. Brearley in June 1914, by Mr. E. Stuart, of R. F. Mosley and Co. Ltd.

Early in 1915 Mr. Brearley left the Brown-Firth research laboratories to become works manager of Brown, Bayley's Steel Works Ltd., the firm with which he was associated until the time of his death. Later he was appointed technical director of the Company. He was also a director of the Firth-Brearley Stainless Steel Syndicate, Chairman of Amalgams Company Ltd. and of Welding Rods Ltd., and a director of the Brearley Ingot Co. Ltd.

He was the author of several technical books, notably "The Case-Hardening of Steel", "The Heat-Treatment of Tool Steel", "Ingots and Ingot Moulds", and "Files and Filing". In 1933 he wrote "Steel-Makers", and in 1946 the American Society for Metals published in America a volume entitled "Talks About Steelmaking", which was originally a series of articles contributed to *The British Steelmaker*. He contributed many other articles to the technical press, and in 1941 he wrote "Knotted String: Autobiography of a Steel-Maker", which was closely followed by further reminiscences under the title "Earning a Living".

In 1920 he was awarded the Bessemer Gold Medal of the Iron and Steel Institute, and in 1939 he was made a Freeman of the City of Sheffield. He became a member of the Institute of Metals in 1919.

There was a very strong humanitarian side to Mr. Brearley's personality. His interest in his fellow-men, their hopes and aspirations, found expression in the formation of the Freshgate Trust Foundation. It was formed, as he himself put it, "to help lame dogs over stiles", or, in other words, to make life more livable for those whose occupations obliged them and their families to live in unlovely industrial areas.

Mr. Brearley is survived by his wife, their only son having died two years ago.

JOHN CHRISTOPHER CLIFTON.

Mr. John Christopher Clifton died in the Farnborough County Hospital on 29 December 1947, following a short illness. He was in his 61st year.

His early technical training was centred around cable manufacture, a subject in which he always retained a lively interest, and his early experiences in this sphere at Siemens Bros. and later at Johnson and Phillips Ltd., both of Charlton, London, were always among his cherished memories.

In 1910 he joined the newly formed metallurgical department of the then Vickers Sons and Maxims Company at Erith, Kent, where he remained until 1930. The company had by then changed to Vickers-Armstrongs Ltd., *via* Vickers Ltd., and had a first-class metallurgical and research department, of which Mr. Clifton was the second in command. In 1931 the merging of the Erith and Crayford Works of Vickers-Armstrongs at Crayford saw Mr. Clifton appointed chief metallurgist, a post he retained until his death. He was elected a member of the Institute of Metals in 1933 and became a Fellow of the Institution of Metallurgists in 1946.

Mr. Clifton was a family man with a very human understanding, which endeared him to all those with whom he was associated. He was a great reader and, outside his own profession, was most interested in medical matters, of which he had a good knowledge.

Throughout his adult life he suffered from a physical disability which made him more of a spectator than a participator in the more active enjoyments of life, and this, together with the all-too-frequent health troubles necessitating surgical treatments in his latter years, gave him a balanced and mellowed outlook which was reflected in his every personal action.

H. G. HARPER.

WILLIAM CULLEN.

Dr. William Cullen died in Edinburgh on 14 August 1948, at the age of 81.

He was educated at Hutcheson's Grammar School, Glasgow, and received his scientific training at the Andersonian College (now the Royal Technical College) and started his career with Nobel's Explosives Co. Ltd., with which firm he was associated throughout his life.

During the Boer War he went to South Africa to join the Modderfontein Dynamite Factory. He became general works manager of the firm in 1901, and was later made a director. He became President of the Chemical and Metallurgical Society of South Africa, and also served as Honorary Secretary of the South African Association for the Advancement of Science.

He returned to the U.K. in 1915 and in that year joined Lord Moulton's staff in the Department of Explosives Supply, later the Ministry of Munitions. After the 1914-1918 war he confined his business activity to consulting work.

He became the representative in London of the University of Witwatersrand, which, in 1925, conferred on him the honorary degree of LL.D. He was elected President of the Institution of Mining and Metallurgy for 1929-1930, and President of the Society of Chemical Industry for 1941-1943. He was also President of the Institution of Chemical Engineers in 1937 and 1938.

He became a member of the Institute of Metals in 1929.

ROBERT LLOYD GIBBINS.

Mr. R. Lloyd Gibbins, J.P., Joint Managing Director of the Birmingham Battery and Metal Company Ltd., died on 31 May 1948.

Mr. Gibbins was born in 1878 and joined the Birmingham Battery and Metal Company Ltd. in 1899. He became secretary of the company in 1908, a

post he held until the centenary of the firm in 1936. In 1911 he was appointed a director of the firm, and from 1911 until 1925 he was largely responsible for the company's works, copper refinery, and laboratory. From 1925 until his death he was in charge of the commercial side of the business as joint managing director. He was a representative of the third generation of his family to manage the company, in which he is now being followed by a member of the fourth generation.

He was particularly interested in the Brass and Copper Tube Association, being its Chairman in 1927 and 1928 and a member of its executive committee from 1925 until his death.

He was elected a member of the Institute of Metals in 1917, and was a Member of Council of the Institute from 1924 to 1934. He served on the Finance Committee of the British Non-Ferrous Metals Research Association for 26 years, and had just retired from that Committee after being its Chairman for 12½ years. He was a keen supporter of the British Non-Ferrous Metals Federation, of which he was Honorary Treasurer from its inception.

He was a bachelor, and lived at Laughern Hill, Wichenford, Worcester-shire. He took a lively interest in the country life of his district.

HENRY HOLME AIREY GREER.

Mr. H. H. A. Greer, J.P., F.Inst.Met., died on 22 November 1947.

He was born in Belfast in 1875. He was in business with his father, Mr. James C. Greer, for 20 years, and afterwards became the sole partner in the firm of James C. Greer and Son, Metal Merchants, of Glasgow. In 1918 Mr. Greer joined the board of directors of A. Cohen and Company Ltd., Great Dover Street, London, but retired some years ago.

An Original Member of the Institute of Metals, he played a large part in the formation of the Scottish Local Section in 1918. He was a member of the committee of the Section from 1918 to 1944, Honorary Local Secretary from 1918 to 1928, and Chairman in 1931 and 1932. He was a Member of Council of the Institute from 1928 to 1936. Mr. Greer was a member of the Reception Committee of the Glasgow meeting of the Institute in 1910, and Honorary Secretary to the Reception Committee in connection with the Institute's very successful Autumn Meeting in Glasgow in 1925.

In the early days of the formation of the British Non-Ferrous Metals Research Association, he was its only member in Scotland, and acted as the Association's Honorary Correspondent in Scotland.

He was well known in the metal trades of Scotland and had been connected with the metal industries for over 50 years. In recognition of his outstanding services to the Institute of Metals for many years he was elected a Fellow of the Institute in 1947.



WILLIAM GEORGE HAYWOOD.

Mr. W. G. Haywood, Managing Director, Haywood Foundries Ltd., London, died on 22 March 1948, at the age of 71.

He was born in London in 1877. He became an electrical engineer and founder, and started in business on his own account as an electrical engineer under the name of W. G. Haywood and Co. in 1902. He acquired a small foundry in 1906, and found it necessary in 1916 to establish this as a separate company under the title of Haywood Foundries Ltd., of which he was principal shareholder and managing director. In 1921, Haywood's N.C.A. Metal Ltd. was formed, manufacturing a corrosion-resistant aluminium alloy introduced by Mr. Haywood. Until the time of his death, he took an active interest in the well-being of all these concerns.

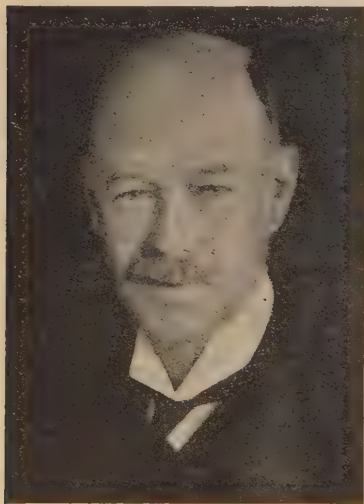
He enjoyed reasonably good health until October 1946, when he underwent an eye operation for glaucoma. The operation was successful and he seemed to make steady progress, but unfortunately he was again taken ill in December 1947, and he died after an illness of eighteen weeks.

Mr. Haywood became a member of the Institute of Metals in 1927.

E. A. BAKER.

SIR WILLIAM MURRAY MORRISON.

When the late Sir Murray Morrison joined The British Aluminium Company in 1894 the world production of aluminium was some 1250 tons, and



when he resigned in 1944 the capacity had reached some 2,250,000 tons. This increase shows not only the progress of the metal, but also serves as an indication of Sir Murray's career. Starting as a civil engineer on the construction of Foyers, Inverness-shire works, he ended his life's work as the managing director and deputy chairman of the company which he had been so instrumental in directing to the position it holds to-day.

He was born at Birchwood in the county of Inverness on 7 October 1873, and trained as an engineer at Edinburgh University and at the Glasgow Technical College, where he studied under Lord Kelvin.

It has been said that Sir Murray was one of the pioneers of aluminium, and this is true in every respect. At the outset there were only the most simple empirical methods of production of alumina, electrodes, and of aluminium itself, the most crude rolling plant, and an entire ignorance of the properties of the metal and of its alloys. Sir Murray realized that the inefficiencies of the various processes were great obstacles to the production of the metal at a cost which would attract consumers, and as soon as he came into a position where his views would be given attention he insisted on research. As a consequence, The British Aluminium Company had its research work directed in turn by Dr. Richard

Seligman, Dr. Bailey, Dr. A. G. C. Gwyer, Mr. G. B. Brook, and to-day by Dr. C. J. Smithells—all prominent in the Institute of Metals.

Sir Murray never lost his keen interest in the technical aspect of the industry, and in later years the managers of the factories were in difficulties, during his visits, as he became so interested in developments which he inspected that his timetable was always delayed.

To-day it is difficult to realize the difficulties faced by Morrison in the early days. The production of the Foyers factory, when he became manager there, amounted to only some 400 tons per year—yet even this amount could not be sold. A part of the power available was turned over to the production of calcium carbide, and efforts were made to produce other materials requiring electrical energy—ranging from diamonds to graphite.

Funds ran low, and the manager had to find money locally from friends and relations in Inverness to pay the weekly wage bill on some occasions. If a workman broke a hammer he went into the woods surrounding the factory and made a new handle. Wages were about 18 shillings per week for a labourer and 25 shillings for a 60-hour week for a furnaceman.

The aluminium furnaces took some 8000 ampères each, and the consumption of alumina, electrodes, and cryolite was very great, as the technique of operation was not only crude, but such things as pyrometers capable of standing up to the fluoride of the bath at temperatures in excess of 1000° C. were not available. Ammeters and voltmeters gave incorrect readings owing to the high magnetic fields in the neighbourhood of the furnaces, and were more likely to cause errors in operation than the foremen's examination of a red-hot poker. Sodium was tapped from the furnaces as frequently as aluminium.

Gradually Morrison was able to sort out the reasons for the many troubles. To insist that improved calcination of the alumina would lead to a reduction in the volume of the fine dust blown off to the air when fed to a furnace. To insist that the oil coke from which the electrodes were made should be calcined to reduce the volatile content to the absolute minimum, and thus to reduce enormously the consumption of carbon.

He became, in association with Lord Kelvin, joint technical adviser to the firm, and in 1910 general manager.

As from 1904, The British Aluminium Company had under construction its Kinlochleven works, which came into operation in 1907 and which were designed primarily by Morrison, so that they incorporated the experience he had gained at Foyers. Here he was able to erect an electrode factory with the most modern plant available at that time.

Furnaces were still of 8000-ampère size, but about 1915 Morrison started increasing the size, first to 16,000, then to 32,000, and finally to 40,000 ampères when the Lochaber (Fort William) reduction works started operation in 1930. Each increase in size brought about a reduction in consumption of electrical energy and a lessening of labour charges, together with improved control, all leading to a better quality of aluminium.

By 1914, when war broke out, the board of directors of the company, acting on the advice of Morrison to a great extent, had acquired interests in Norway for the production of aluminium, as the power costs were low. This led to a new alumina factory at Burntisland in Fife, to help the original factory at Larne, whose capacity was being strained. Here Morrison was able to turn his attention to the creation of an alumina factory in line with modern ideas and embodying the results of his observations at Larne. By 1938 the increase in size of the Lochaber reduction works and the increased demand for alumina necessitated the building of a new alumina works at Newport, Monmouthshire.

Sir Murray had broadened of necessity from a purely technical man—an engineer—to a business man, but fundamentally he retained always his very great interest in hydro-electric developments and all processes leading to the

production of virgin aluminium, and could always be tempted away from pure business to discuss technical matters with his managers and scientific men.

It had been recognized very early by the makers of aluminium in France, Switzerland, and the U.S.A. that if the metal was to sell at all, it was necessary for them to operate their own rolling mills, so that consumers could be given metal ready for use. For many years the rolling mills lost money, and it was really the impetus given to aluminium by the discovery of Duralumin, and by the advent of aircraft, that brought in aluminium fabricators who were not concerned with the making of the virgin metal.

The company, starting with an old mill at Milton (Staffordshire) and the first extrusion press, found necessary a second mill at Warrington, as the demand grew, and Morrison came into this picture as well. He did not pretend to know the details of rolling, but was insistent on discussing the planning of lay-out. In fact, his technical department found him difficult to deal with, as he was firmly of the opinion that more companies had run into financial difficulties through too much expenditure on bricks and mortar than from other causes. He therefore required a great deal of convincing that space was necessary between machinery to handle the bulky products.

The 1939 war brought about an increase in the capacity of both Warrington and Milton, and led to the construction of a large new mill at Falkirk in Stirlingshire.

It gave great satisfaction to Sir Murray that this mill had been set up in Scotland at the request of the Ministry of Aircraft Production, because he had then created for his beloved Scotland a complete industry, starting from alumina and ending with the rolled product. During the war a factory was erected for the Ministry of Aircraft Production at Latchford, Warrington, for the production of aluminium powder required for use in explosives and for the treatment of scrap resulting from the large aircraft industry.

Apart from the increase in number and size of the factories in Great Britain and in Norway, Morrison was instrumental in developments overseas, ranging from virgin-metal production to finished products, all of which linked up with the activities of the company and were necessary to protect the company, and in 1933 he was made a Commander of the Order of St. Olav of Norway. He was knighted in 1947 for his services to Great Britain.

He was a Member of the Institution of Civil Engineers, a Member (and past Member of Council) of the Institution of Electrical Engineers, a Fellow of the Institute of Physics, a past Member of Council and Vice-President of the Faraday Society, and a Vice-Chairman of the British Non-Ferrous Metals Research Association.

Sir Murray was an Original Member of the Institute of Metals, a Member of Council from 1912 to 1923, and a Vice-President from 1923 to 1928. He was elected a Fellow of the Institute in 1937 and was awarded its Platinum Medal in 1942.

He is survived by his wife and daughter.

G. BOEX.

SIR CLIFFORD PATERSON.

Sir Clifford Paterson, O.B.E., D.Sc., F.R.S., died on 26 July 1948, after a short illness, at the age of 68.

Clifford Copland Paterson was born on 17 October 1879 at Stamford Hill, and was educated at Mill Hill School. After serving a four years' engineering-shop apprenticeship, he passed through the Finsbury Technical College and Faraday House. The first part of his career from 1901 to 1918, was at the National Physical Laboratory as principal assistant under Sir Richard Glazebrook, responsible for the electro-technical and photometric departments.

He joined The General Electric Co. Ltd. in 1919 to establish and direct the G.E.C. research laboratories. Under his guidance, these laboratories, which began with a staff of 29, developed to their present size, with a staff of 1250. He was appointed to the board of the G.E.C. in 1941 and received his knighthood in the Birthday Honours List of 1946. He was a Past-President of the Institution of Electrical Engineers, of the Institute of Physics, of the Illuminating Engineering Society, and of other scientific bodies. He was Chairman of the Council of the British Standards Institution and a member of the Advisory Committee of the Department of Scientific and Industrial Research. He served as a member of the Executive Committee of the National Physical Laboratory, and as Vice-President of the Royal Institution and of the Royal Society of Arts. He received the honorary degree of D.Sc. from Birmingham University in 1937, and he was for two years Master of the Worshipful Company of Tallow Chandlers. The last of many honours which he received during his life was the award of the Gold Medal of the Illuminating Engineering Society (America), which was presented to Lady Paterson on Thursday 22 July 1948, by Preston Millar, Past-President of the Society.



Sir Clifford had just returned from Australia, where he had been to establish liaison between scientific bodies of Great Britain and Australia.

He was elected a member of the Institute of Metals in 1919.

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